Three Essays on Risk Assessment Instrument Policies Across State Criminal Justice Systems

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ABSTRACT

Stefany Yolanda Ramos: Three Essays on Risk Assessment Instrument Policies
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(Under the direction of Rebecca Kreitzer)

In the criminal justice system, risk assessment instruments (RAIs) provide information about justice involved individuals that court officials use to make various decisions. This dissertation explores the adoption and impact of state-level policies regarding the use of risk assessment instruments. This is the first study to collect detailed data on state RAI policies across all 50 states. The first chapter explains the distinction between research on assessment instruments and research on assessment policy, and details the data collection process. I find that many states adopted RAI policy while recovering from the Great Recession and also facing strong criticism for police brutality against people of color. The second chapter examines the motivations for policy adoption using event history analysis. Neither economic nor social factors had an effect on the likelihood of RAI policy adoption. Furthermore, while party values suggest adoption of RAIs for different purposes, I find no evidence that party moderated the influence of economic or social indicators. Results suggest that RAI policies were passed for political purposes rather than to address specific needs.

The third and fourth chapters examine the impact of RAI policy. The third chapter evaluates the effects of state-level policies on prison populations. Using several difference in differences models I find evidence that RAI policies decrease imprisonment rates under the right circumstances. Average treatment effects show null results; however, group-time and cohort treatment effects provide insight into when and where there was a strong response to RAI policy. Policy effects were stronger for early adopters; however, duration models do not support a dosage

effect, suggesting that place-time context is key. The final chapter examines policy effects on racial disparities in sentence length for new prison admissions. Using ordered logistic regression, I find that policies which mandate RAIs for some offenders at the time of sentencing reduce the probability of receiving long sentence lengths for both Black and White offenders convicted of violent crime, and eliminate racial differences in sentence length for violent crime. While most recent criminal justice reform has been focused on non-violent and drug crimes, RAI policies at sentencing may be helpful in addressing incarceration rates and racial equity among violent offenders.

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LIST OF ABBREVIATIONS

ARRA	American Recovery and Reinvestment Act of 2009
ATT	Average Treatment effect on the Treated
BLM	Black Lives Matter
DI	Dissimilarity Index.
EHA	Event History Analysis
NCRP	National Corrections Reporting Program
RAI	Risk Assessment Instrument
TWFE	Two-Way Fixed Effects

CHAPTER 1: INTRODUCTION

This dissertation explores the adoption and impact of state-level policies regarding the use of risk assessment instruments (RAIs) in the criminal justice system. RAIs identify people who pose high (and low) risk to public safety. They use strict mathematical models that produce a statistical prediction of risk and a final risk score. They aid in decision-making all along the path of criminal procedure, through incarceration, release and reentry. Risk assessments have evolved, but have been commonplace in some form throughout the United States for quite some time. What is new is the involvement of the far-reaching hand of the state. I show that by 2018, forty states had passed legislation mandating administration of RAIs for some offenders. State-wide RAI policies impose a formal structure to what was an informal norm, structured only at the local level and by local officials. Ideally, the new policies would have undergone a stringent evaluation process before wide-spread adoption. We should have an understanding of policy impact compared to business as usual and feel confident that there are no iatrogenic effects. This is an important principle even during a trial or evaluative phase of policy development (MacKay, 2018). However, the impact of RAI policy is yet unknown. My research builds on previous work to investigate whether RAI policy is better than the status quo.

Scholars have not reached consensus on the value of risk assessments for justice and public safety. Risk assessment literature largely provides evaluation of specific assessments and in specific jurisdictions[1]. A second body of work discusses the accuracy and equity of the algorithms that

[1] For example, DeMichele, Baumgartner, Wenger, Barrick, & Comfort, 2020; Ostrom & Kauder, 2013; M. Stevenson, 2018

produce risk scores from a mathematical or theoretical perspective[2]. Both ask questions about the accuracy of predictions, or whether predictions have the same failure rate across races for example. A third line of investigation examines changes in judicial discretion and offender outcomes when RAIs are used[3]. These studies suggest that the value of RAIs is dependent on how they are used. Support for risk assessments has wavered. Yet because of legislation, they are permanent fixtures of state systems. I offer three unique contributions to the literature on risk assessments and justice outcomes. First, I investigate policy rather than instrument or practice. Second, my analysis includes all 50 states to understand broader impacts. Third, I take an interdisciplinary approach to the study of criminal justice.

There is a distinction between the effect of risk assessment instruments and the effect of risk assessment policies on justice outcomes. Literature on the two subjects typically differ in scope, sample, and level of analysis. Instrument studies often sample among the population of individuals who receive a risk score via the instrument. They are usually at the offender-level, and limited to one or a few jurisdictions using the same instrument. The research question is usually concerned with outcomes as a result of characteristics of the assessment itself, or of assessment implementation. In contrast, policy studies are typically concerned with outcomes at a jurisdictional level, and sample from the population of jurisdictions. The data itself may be at the jurisdiction- or offender- level. The scope of these studies may be wider due to the nature of the research question or the accessibility of data. I measure outcomes at a jurisdictional-level and my counterfactual is the absence of RAI policy.

[2] See Andrews, Bonta, & Wormith, 2016; Barabas, Dinakar, & Doyle, 2019; Douglas, Pugh, Singh, Savulescu, & Fazel, 2017; Helmus & Babchishin, 2017

[3] For example, Sloan, Naufal, & Caspers, 2018; M. T. Stevenson & Doleac, 2019.

I use literature from criminology, political science, economics, and public health to evaluate RAI policy. Public policies operate in wide-ranging circumstances and to various degrees of fidelity. Outcomes under public policy may be significantly different than outcomes under study conditions. Public policy scholars use a variety of interdisciplinary theories and tools to determine impact. The fields of education and health have embraced this approach but criminal justice research remains somewhat siloed (Pickett, 2019). My work shows that an interdisciplinary approach enhances our understanding of criminal justice and its relationship with other fields.

The remainder of this chapter will explain RAIs in greater detail and describe the policy data collection process. I use those data to investigate policy effects across the United States. The second chapter investigates the motivations for the wide-spread adoption of the policies. We cannot begin to understand this policy intervention unless we know where to look. To date there exists no compilation of RAI policies across the United States. I built an original dataset of state RAI policies from 1994 to 2018. I find that many states adopted RAI policy while recovering from the Great Recession and also facing strong criticism for police brutality against people of color. However, neither economic nor social factors had an effect on the likelihood of adoption for either Republican or Democratic led states. These and other results suggest that RAI policies were passed for political purposes rather than to address specific needs.

The third and fourth chapters examine RAI policy effects. The third chapter evaluates the impact of state-level policies on prison populations, and asks whether RAI policies reduce imprisonment rates. I find evidence that RAI policies decrease imprisonment rates under the right circumstances. The final chapter examines the ability of RAI policies to address racial disparities in sentence length for new prison admissions. I find that policies which mandate RAIs at the time of sentencing for some offenders eliminate Black and White offender differences in sentence length for violent crime.

1.1 Risk assessment instruments

Generally, risk assessments provide court officials with information to help determine whether the defendant/offender should be released or punished, and the severity of the punishment. These tools are used at different points in the system. Table 1-1 summarizes the purpose of the assessments at each stage. It is helpful to imagine one individual as they move through the system. After arrest, a pretrial RAI helps the court decide whether the individual should be held or released pending trial. If the defendant is convicted, a sentencing RAI may guide the decision to divert the offender to alternative programming, community supervision, or to prison. If they are sentenced to prison, a prison RAI is used to determine the type of facility and custody level. If they are sentenced to community supervision or after they complete their prison term, a probation/post-release RAI guides planning the intensity of supervision, and some include an individualized case management plan. If they are still serving a sentence in prison but eligible for parole, a parole RAI aids a parole board in deciding to release or deny parole. One individual may encounter none, one, or all of these assessments. Some states have adopted a system of RAIs, while others only mandate their use at certain points, leaving local jurisdictions to design the rest of the process.

There are dozens of specific assessments, developed by both public and private organizations, though some are more popular than others. Three of the most widely used RAIs are the COMPAS, the Ohio Risk Assessment, and the Public Safety Assessment. They differ in the risk factors used to model risk and have evolved over several decades. The first generation of assessments were not actuarial and relied simply on professional judgement. In the 1970s, mathematical measures that included specific risk factors were developed. These second-generation assessments were the first actuarial instruments, some of which are still used. Their risk factors are static, for example age, gender, and criminal history. Third generation RAI risk factors are more grounded in criminogenic theory and include dynamic factors such as attitude and beliefs. Fourth generation RAIs use similar factors but also include case planning and risk management. Third and fourth generation assessments are often automated and can be complex.

1.2 RAI policy and data collection

I define a RAI policy as any law regarding the use of RAIs as a matter of standard criminal procedure. I examine state-level policies, excluding assessments that target specific populations. I created a panel dataset of RAI policies by state from 1990 to 2018. I searched LexisNexis Advance® for session laws[4] referencing risk assessments in the criminal justice system. I cross-referenced and supplemented that collection with reports from research organizations and state websites. To be included in this dataset the law must specifically mention the assessment of risk. For example, laws that require medical assessment, or assessment of rehabilitation needs are excluded. I also exclude laws requiring RAIs specific to one type of crime. For example, risk assessments designed for juveniles, for sexual violence, or for domestic violence are common but are not included. These risk assessments predict specific actions and are fundamentally different than the generalized RAIs I have described in detail. A summary table of the dataset is available in Appendix A.

[4] Session laws are published at the end of each session as a collection of statutes enacted by the legislature. The advantage of using session laws for an adoption study is that they reflect the laws as they were adopted without amendments from subsequent laws.

1.3 Tables

Table 1-1. Summary of risk assessment instruments

When assessment is used	Purpose of assessment
Pretrial detention	Helps the court decide which defendants can be released pending trial
Sentencing	Sentencing judges use risk assessment to decide whether prison or alternative programs are necessary and the length/severity of the sentence
Prison	Used to determine the type of facility and custody level
Probation/Post-release supervision	Probation officers use the risk assessment to decide on the intensity of supervision, and to develop an individualized case management plan
Parole boards and releasing authorities	Helps parole board decide which prisoners to release

CHAPTER 2: RECESSION, STRUCTURAL INEQUALITY, AND PARTY: THE DETERMINANTS OF RISK ASSESSMENT POLICY ADOPTION IN STATE CRIMINAL JUSTICE SYSTEMS

2.1 Introduction

Risk assessment instruments (RAIs) have been a common practice of the criminal justice system for over a century. RAIs identify high and low risk offenders to aid decision-making. However, in the last few decades they have been enshrined in state law. Virginia was the first to mandate RAIs in 1994 (Code of Virginia, 17.1-803). They were included within a package of criminal justice reform aimed at reducing the state's prison population by 25%. Few states followed suit, instead passing three-strikes laws and other draconian policies that filled prisons. But in the 2010's, for unknown reasons, RAIs attracted attention from policymakers and more states adopted RAI laws. This movement towards a centralized system is an important shift. Though a great deal of flexibility remains, state adoption brings with it some level of standardization to RAI implementation as jurisdictions are forced to conform to state law. Now there is a movement to investigate their effectiveness and ensure they do not replicate or exacerbate structural inequalities that already permeate the justice system. An important benefit of state-level analysis is the ability to estimate population-level policy effects. However, our understanding of policy impact is limited by our understanding of how many and which states have adopted the policy. This study is the first to provide a state-level map of RAI policies and examine motivations for adoption.

The unique environment of the early 2000s through 2018 may have led economic and social problems to a shared solution. According to Jones and Baumgartner's extensive study on policy attention, criminal justice policies share agenda space with several other policy areas, a phenomenon called agenda crowding or policy spillovers (Jones & Baumgartner, 2005). This is when attention to one policy area is positively correlated with legislative action in another policy area. Crime policy and high salience of economic conditions are positively correlated (Jones & Baumgartner, 2005, p.256). Although their study examines national data and federal policy, it is reasonable to expect that during a time of economic crisis, like the Great Recession, governments might respond by addressing criminal justice. On the other hand, crime policy and civil rights attention are negatively correlated, suggesting that when racial tensions are high governments are *less* likely to pass crime policies. However, civil rights issues were directly tied to law enforcement's abusive treatment of Black and Brown communities. These factors point towards criminal justice reform as a natural target for civil rights activists of the time. I examine whether states struggling with economic and social challenges were more likely to adopt RAI policy. I also examine whether party moderated economic and social influence on the probability of adoption.

2.1.1 Economic decline and recovery

The subprime mortgage crisis in 2006 signaled the start of economic decline in the US. Homes lost value, and banks began to falter. Lehman Brothers declared bankruptcy, and the stock market crashed. The Great Recession lasted from December of 2007 to June of 2009. By that time the net worth of households dropped from $69 trillion to $55 trillion, and unemployment rose to 10% (Rich, 2013). It was ended by the American Recovery and Reinvestment Act of 2009 (ARRA) which pumped $787 billion into the economy. Ten years later, national economic indicators of recovery were mixed. The unemployment rate recovered well, dropping down to just 4% in December of 2017. Long term unemployment, however, remained high. In 2007, about 17.6% of people unemployed were looking for work for 27 weeks or more. In 2017, the proportion increased to almost 25% (Cunningham, 2018). Furthermore, *involuntary* part-time employment was higher than it was before the recession, suggesting that those who did gain employment were not able to find the full-time positions for which they had hoped. Median incomes were the same or worse than they were in 2000 for people with lower levels of education and for racial minorities (Cunningham, 2018).

At the state level, fiscal recovery varied widely. Federal aid from ARRA played a key role in reducing unemployment and curbing further economic decline. The share of state revenue that came from the federal government jumped about 10 percentage points from 2008 to 2010 in Tennessee, Alaska, and New Mexico, while the share in states like Wyoming, Louisiana, and North Dakota barely changed (Pew, 2019). As aid from the Recovery Act ended, states had to replace those funds with alternate sources of revenue such as taxes. But in 2014, 29 states still collected less tax revenue than they did at their peak quarter before the end of the recession (Pew, 2019).

Meanwhile, 1 in 100 people experienced the Great Recession behind bars. In 2008, 3.1% of the population, or 1 in 32 adults, were under some form of correctional supervision (Maruschak & Minton, 2020). Incarceration rates were the highest they had ever been at an average of 442 per 100,000, and as high as 862 in the state of Louisiana. Mass incarceration was (and remains) costly to state budgets. Spending on corrections rose 127% between 1998 and 2008 (Warren, 2008). As in Virginia, risk assessments were used in efforts to reduce the prison population. With risk assessments, states may reserve prison for individuals who have a high risk of reoffending. Alternative programming and community supervision are less costly than prison, thus more offenders serving alternative sentences and fewer offenders in prison may save states money. If RAIs were considered cost saving measures, policy adoption should be negatively associated with tax revenue, and positively associated with federal revenue and spending on corrections.

2.1.2 Racial inequality and social unrest

It is well documented that racial minorities are disproportionately affected in all aspects of the criminal justice system, Black males in particular. Black individuals are punished more often and more severely in relation to traffic stops (Lundman & Kaufman, 2003; Rojek, Rosenfeld, & Decker, 2012), drugs (Lyons, Lurigio, Roque, & Rodriguez, 2013), sentencing decisions (Bushway & Piehl, 2009; Spohn & Cederblom, 1991), and capital punishment (Baumgartner, Davidson, Johnson, Krishnamurthy, & Wilson, 2018; Sarat, 2006). High rates of incarceration in neighborhoods contribute to weakened familial and community relationships, and increase unemployment and criminal involvement (Crutchfield & Weeks, 2015). Communities of color have borne the brunt of this systemic disassembly of social and economic infrastructure. In 2008, 1 in 9 Black men between the ages of 20 and 34 were locked away in jail or prison, compared to 1 in 30 White men in the same age group (Warren, 2008).

Just as many states started fiscal recovery from the Great Recession, public outrage and protest erupted in response to a series of high-profile deaths of Black individuals due to lethal interactions with law enforcement. Figure 2-1 displays a sample of murders that garnered attention. It is by no means a comprehensive account of deadly encounters with police but illustrates the quick succession of events which heightened awareness of injustices that many had been trying to point out for decades. Non-lethal discrimination also drew attention, especially for powerful figures. For example, in July of 2009, Dr. Henry Luis Gates, a distinguished Harvard University professor at the time, was arrested trying to enter his own home in Cambridge, MA. Even President Obama was subjected to persistent allegations that he was not a U.S citizen by a movement trying to delegitimize his presidency.

Calls for action were not solely in response to isolated traumatic incidents. They urged acknowledgement and restitution for centuries of racial violence embedded in American law. The Black Lives Matter movement, or BLM, was established in 2013, right after the man who shot and killed 17-year-old Trayvon Martin was acquitted of murder charges. According to their website, the purpose of BLM is to fight against White supremacy, and systematic oppression and violence against Black people. Advocates point out racial disparities in housing, economic stability, and mass incarceration. The 20/20 Bipartisan Justice Center, a group of Black Republicans, Democrats, and Independents, was established in 2015 specifically to influence policy. Through listening forums and partnerships with Google, Facebook, and BET they called on political candidates to identify their platform for criminal justice reforms. Protests in public arenas and on social media put pressure on policymakers to act. RAIs curb judicial discretion by standardizing assignment of risk levels. Social justice advocates hoped that this would help relieve racial bias in court official decision-making. If RAIs were considered a tool to address bias and systemic disparate outcomes, then RAI policy adoption should be positively correlated with measures of racial inequality.

2.1.3 RAI policy adoption and politics

Scholars of political science who study the way policies spread have shown that states look to neighboring states, states in their geographical region, and states with similar ideologies for guidance on good governance, and to identify polices that are politically advantageous (Berry & Berry, 1990; Graham, Shipan, & Volden, 2013; Grossback, Nicholson-Crotty, & Peterson, 2004; Mooney, 1999; Volden, 2006). While RAIs enjoyed bipartisan support, the rhetoric regarding their benefits differed by party. All political parties prioritize the economy. Republicans, however, promote fiscal restraint. Anecdotal evidence suggests that Democrats are not only concerned about the economy but also interested in addressing social problems. Democratic senators often reference equity in defense of criminal justice bills. For example, Dick Durbin (D-IL) praised the First Step Act[5] as "a victory for those who want to make sure that we have a just system when it comes to criminal law" (Durbin, 2018). Senator Corey Booker (D-NJ) has said that mass incarceration "cost taxpayers billions of dollars, drained our economy, compromised public safety, hurt our children, and disproportionately harmed communities of color while devaluing the very idea of justice in

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[5] The First Step Act increased the cap on "good time" credits and made crack cocaine sentencing reforms from a 2010 law retroactive leading to the release of over 3,000 prisoners and a reduction in sentence for another 1,600 (Lau, 2019).

America" (Booker, 2018). Since both parties are affected by the economy, I expect that the effect of economic indicators on the probability of RAI policy adoption will not differ by party. However, indicators of racial inequality should have a stronger effect when state governments are Democratic.

2.2 Methods

Once a local decision, the ability to analyze policy effects on a large scale was difficult since there are thousands of jurisdictions, each with their own requirements. However, information about state-level policies is much more manageable in terms of data collection. I developed an original dataset of state-level RAI policies from 1990 to 2018. A summary table of the dataset is available in Appendix A. I show that forty states adopted RAI policy by 2018. Figure 2-2 shows the cumulative percent of the United States with state-level RAI policies. From 2010 to 2018, the percentage of Americans living under state-wide RAI policies rose from 6% to over 78%[6]. For this study, I use data from 2006 to 2018 to focus on the period surrounding the Great Recession through the latest available data. This timeframe coincides with the trend towards state RAI policy adoption.

To assess the economic condition of each state-year I use indicators of fiscal health frequently used by economists and especially useful to evaluate recovery from the recession. Table 2-1 provides a list of variables and sources. The percent of state revenue from the federal government was unusually high because of funds from the Recovery Act, and higher in states that suffered more. As those funds expired and states regained their own footing, they were able to increase revenue from taxes. I use the difference between state-year tax revenue and tax revenue during the state's peak quarter before the end of the recession as an indicator of recovery. I also use the percent of total expenditures spent on corrections as a direct measure of economic pressure due to high incarceration rates.

[6] Proportions are similar for Black and white populations, rising from 5.28% to 80.88% and 6.55% to 78.21%, respectively.

To assess the state of racial inequality I use several measures of structural racism from public health literature (Groos, Wallace, Hardeman, & Theall, 2018; Lukachko, Hatzenbuehler, & Keyes, 2014; Mesic et al., 2018). I have already discussed racial disparities in incarceration rates. The Black to White incarceration rate ratio is the first measure of racial inequality. During the study period, the Great Recession had a disproportional financial impact on Black families. Between 2005 and 2009, the median net worth of Black households dropped by 53 percent, while White household net worth dropped by 17 percent (Tippett, Jones-DeWeever, Rockeymoore, Hamilton, & Darity Jr, 2014). I use the Black to White median income ratio as a second measure of racial inequality. Finally, I use a measure of racial residential segregation. Residential segregation is increasingly identified as a cause of health, educational, and employment disparities between races (Williams & Collins, 2014). I use the Dissimilarity Index (DI) which indicates the proportion of Blacks that would have to change their place of residence to achieve an even distribution of Whites and Blacks in the region. The DI is calculated as follows:

$$DI = .5 * \sum_{i=1}^{n} | x_i/X - y_i/Y$$

where n is the number of counties in the state, x_i is the count of Black residents in the county, X is the count of Black residents in the state, y_i is the count of White residents in the county, and Y is the count of White residents in the state[7].

To measure the direct impact of violence and crime I include the incarceration rate, violent crime rate, and property crime rate. I expect the probability of RAI policy adoption to increase as incarceration and crime rates rise. State-level controls include the proportion of Black residents in

[7] I use county level population estimates from the National Cancer Institute: Surveillance, Epidemiology, and End Results Program which stores county-level Black and White population data compiled by race and age from 1969-2018. County level data are preferred to census tract data in this case because the DI is highly influenced by small populations. Counties with less than 10,000 population total or less than 1,000 minority population were removed.

the state, the unemployment rate, and per capita income (logged). I also estimate the effect of external factors using the proportion of neighboring states that previously adopted a RAI policy, and the census division[8]. Table 2-2 shows means for all covariates with standard deviations in parentheses. There are 650 state-year observations in the full dataset. Column 1 is the full sample of state-years. Columns 2 and 3 display means for state-year without and with RAI policies, respectively. Columns 4 and 5 display means for states with Republican and Democratic governors, respectively. Column 6 displays the p-value from a comparison of means between Republican and Democratic state-years. State-year observations without RAI policy had more Democratic legislatures, and a larger difference between state-year tax revenue and peak tax revenue on average. Observations without RAIs had a higher Black to White incarceration rate ratio on average. They also had higher violent and property crime rates. On average they were Whiter, less employed, and less wealthy than observations with RAI policy. The difference in means between state-years with Republican and Democratic governors were statistically significant for nearly all variables.

I used event history analysis (EHA) of first-time adoption of RAI policies to investigate adoption motivations. In the main analysis, the dependent variable is a dichotomous indicator of any RAI policy adoption in that state in that year. Following standard EHA practice, state-years after the year of adoption were removed. The final dataset for analysis included 412 state-year observations from 2006 to 2018. I included year fixed effects to account for other factors that may have made RAI policies more attractive in a particular year, for example in 2011 when there is a spike in adoption. To estimate party moderation all economic, social, and public safety variables

[8] Year fixed effects precluded me from using a measure of total previous adopters as is common in diffusion literature. Therefore, any effects from previous adopters is captured in the fixed effects but cannot be isolated.

were interacted with party as measured by the percent of Democrats in the state legislature[9]. Robust standard errors are clustered by state-year to account for non-independence of errors.

2.3 Results

Results of EHA analyses are displayed in Table 2-3. Model 1 does not include interactions for party moderation. Per capita income (logged) is the only internal measure that was significant and had a negative effect on the likelihood of RAI policy adoption. This suggests that wealthier states had less incentive to impose this intervention on their criminal justice system. The proportion of neighboring states also had a negative effect on the likelihood of adoption. This is somewhat unexpected as diffusion studies often find that states follow their neighbors. When the interactions were added, in model 2, the negative neighbor effect grew stronger. This suggests that neighbor adoption was a deterrent. Certain regional divisions were more likely than others to adopt RAI policies, namely the West South Central and Pacific regions[10].

Economic measures had no effect on the likelihood of adopting RAI policy. The hypothesis that state fiscal needs were an incentive for adoption is not supported. Among the racial inequality measures, DI had a positive effect on the probability of adoption and was the only statistically significant finding. Joint tests of significance fail to reject the null hypothesis. This offers only weak support for the hypothesis that large systemic racial disparities were an incentive for adoption. In addition, no economic or inequality interactions with party had an effect. The hypothesis that party moderated the effect based on party values is not supported.

Neither violent nor property crime rates influenced the probability of adoption. This is unsurprising given that crime rates are also uncorrelated with incarceration rates. In contrast,

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[9] I also tested the model using 1) an indicator of Democratic governor as the party measure, and 2) using an indicator of party in power, both with similar results.

[10] The West South Central division includes Arkansas, Louisiana, Oklahoma, and Texas. The Pacific division include Alaska, California, Hawaii, Oregon, and Washington.

incarceration rate had the strongest internal effect. An increase in the incarceration rate was associated with a reduction in the probability of adoption. Moreover, the effect is moderated by party. Figure 2-3 shows the average marginal effect of incarceration rate as the proportion of Democrats in the legislature changes. The effect remains negative until the legislature reaches about 70% Democrats. However, during this period the maximum proportion of Democrats was 61%.

Figure 2-4 shows the predicted probability of adoption in three scenarios: 1) at the mean proportion of Democrats, 50%, 2) at one standard deviation below the mean, 33%, and 3) at one standard deviation above the mean, 68%. When incarceration rates are low legislatures that are 2/3 Republican have a 50% probability of adoption, while legislatures that are 2/3 Democratic have only 9% probability of adoption. The steep downward slope of the dotted line shows the marginal effects of incarceration rates are much larger in majority Republican states. In contrast, the slope of majority Democratic legislatures, the solid line, is almost flat. At high incarceration rates the probabilities for all three groups almost converge between 7 and 8%.

2.4 Discussion and implications

Although the main hypotheses are not supported, the null results are telling. There is no evidence that RAI policies were mandated as a mechanism for cost saving. Likewise, there is no evidence that the policies were adopted to address racial inequality. Party did not moderate economic or social effects. Political rhetoric around criminal justice reforms often played to party values – fiscal responsibility for Republicans, and social justice for Democrats – however, RAI policy adoption did not fall along those lines. It is possible that legislators opted to back fewer policies with larger changes to address economic and social need, rather than using political capital to pass policies with indirect and incremental change, such as RAIs. Given the severity and persistence of both the Great Recession and structural racism, states may have been focused on direct and perhaps more drastic policies.

Incarceration rates, which would be directly affected by RAI policies, were the strongest determinants of adoption. Interestingly, Republicans were influenced by incarceration more than Democrats. Furthermore, Republicans were most likely to adopt when incarceration rates were low, not high. Democrats on the other hand were unmoved by changes in incarceration rates. Studies show that conservative governments are more likely to be influenced by the political consequences of policy adoption, while liberal governments are more influenced by policy effects (Gilardi, 2010). Since there are few studies about the broader impacts of RAI mandates it is difficult to make any statement of impact with certainty, leaving arguments wide open to opportunistic conjecture. Perhaps adoption was less about the impact of RAIs, and more about claiming credit for "taking action." Given the high rate of adoption across the country, over 80% of states, RAI policies may represent the low hanging fruit of criminal justice reform, a signal of acquiescence to placate demands for change.

The economic, social, and political indicators here do not capture motivations for RAI policy adoption. In some ways the bipartisanship of RAI policy represents a return to reform efforts a decade earlier. In the late 2000's a few pieces of legislation – the Second Chance Act and the Fair Sentencing Act – made their way to the President's desk in a rare show of bipartisanship. These two federal laws provided funds for reentry programs and shortened sentences for crack cocaine to reduce prison populations and recidivism. They signaled a change from decades of policies imposing harsh and swift punishment in eras characterized by a focus on incapacitation, individual responsibility, and just deserts. But attention abruptly turned to the economy as the United States entered the Great Recession. State budgets were devastated by the economic decline, and they were forced to borrow funds from the federal government and make difficult decisions to cut spending where they could. Just as economies began to improve, growing social unrest led to mass protests against police brutality and the nation once again turned its head towards the criminal justice system.

Communities were tense and governments frustrated by a social crisis that detracted attention from economic recovery. The Smart on Crime initiative led by Attorney General Eric Holder dedicated funds for evidenced-based practices that might save money, address racial bias, and restore trust in the system. Amidst these competing forces, risk assessments gained popularity as tools that could do just that.

However, state risk assessment policies lack the proactive measures of those early federal laws. Instead, they are procedural changes that reflect no obvious reformist stance and are highly dependent on implementation. Even so, they could still make a significant impact on the composition of incarcerated and community populations. If they accurately identify high risk individuals with equitable outcomes many offenders may avoid prison altogether, staying instead in their communities under supervision or in treatment programs. This in turn may affect public health and economic outcomes for both the individual and their families. Communities of color continue to experience high incarceration rates. Racial tensions have only intensified after a presidential administration that refused to acknowledge systemic racism and bolstered White supremacist conspiracy theories. With mandates in place, states will need to focus on enforcement and oversight. Then we may begin to understand whether risk assessment policies help address economic and social needs.

2.5 Tables

Table 2-1. EHA model variables and sources

Construct	Measure	Source
Economy	% of federal revenue	Fiscal 50, Pew Charitable Trusts
	% tax revenue	Fiscal 50, Pew Charitable Trusts
	% expenditures on corrections	Annual Survey of State Government Finances
Racial inequity	Dissimilarity Index	Author construction
	Black/White incarceration rate ratio	Author construction from National Prisoner Statistics and US Census
	Black/White median income ratio	Author construction from American Community Survey
Public Safety	Incarceration rate	National Prisoner Statistics, Bureau of Justice Statistics
	Violent crime rate	Universal Crime Reports, FBI
	Property crime rate	Universal Crime Reports, FBI
State controls	% Black residents	American Community Survey 1-year estimates
	Unemployment rate	American Community Survey 1-year estimates
	Per capita income (logged)	American Community Survey 1-year estimates
Diffusion	% of neighbors with policy	Author construction
	Geographical division	US Census
Party	% Democrat in state legislature	National Council of State Legislatures

Table 2-2. Descriptive statistics of adoption model covariates

Variable	(1) Full sample	(2) Full sample RAI = 0	(3) Full sample RAI = 1	(4) Republican governor	(5) Democrat governor	(6) Party difference p-value
RAI	0.40 (0.49)	0 (0)	1 (0)	0.40 (0.49)	0.39 (0.49)	0.64
Percent dem legislature	47.28 (18.03)	50.57 (17.41)	42.26 (17.83)	41.03 (18.17)	55.17 (14.07)	<.001
Percent federal revenue	31.50 (6.08)	31.97 (6.29)	30.96 (5.70)	32.13 (6.32)	30.67 (5.65)	0.002
Difference from peak tax revenue	-2.86 (16.28)	-4.87 (17.72)	-0.09 (13.62)	-3.67 (17.67)	-0.60 (9.97)	0.01
Median income (2018 dollars)	59102.54 (9804.35)	59052.82 (9943.75)	59170.77 (9627.45)	57474.05 (9266.87)	60963.81 (10037.73)	<.001
Percent corrections spending	2.42 (0.65)	2.45 (0.66)	2.38 (0.65)	2.36 (0.61)	2.49 (0.04)	0.01
Dissimilarity index	31.83 (10.45)	31.07 (10.75)	32.98 (9.89)	30.97 (10.06)	33.02 (10.96)	0.01
B/W incarceration rate ratio	6.96 (2.90)	7.21 (3.01)	6.62 (2.94)	6.68 (2.96)	7.35 (3.03)	0.01
B/W median income ratio	0.64 (0.12)	0.64 (0.13)	0.63 (0.9)	0.63 (0.12)	0.65 (0.11)	0.02
Incarceration rate	426.04 (157.79)	426.18 (168.15)	425.83 (140.80)	449.35 (163.16)	394.09 (144.51)	<.001
Violent crime rate	378.68 (152.27)	384.17 (164.40)	370.29 (131.45)	385.64 (154.13)	364.99 (141.39)	0.07
Property crime rate	2800.72 (715.43)	2892.94 (723.69)	2659.87 (680.11)	2798.02 (724.43)	2800.56 (710.06)	0.96
Percent Black	10.37 (9.49)	9.18 (9.27)	12.01 (9.56)	11.29 (10.48)	9.32 (7.99)	0.01
Unemployment rate	5.84 (2.15)	6.03 (2.19)	5.53 (2.05)	5.70 (2.18)	5.96 (2.07)	0.11
Per capita income (2018 dollars)	47898.93 (7767.61)	47139.70 (8204.00)	49058.06 (6904.04)	46815.46 (7270.53)	49128.1 (8165.58)	<.001
N	650	423	277	392	301	

Note: Standard deviations are in parentheses. Column 1 is the full sample of state-years. Columns 2 and 3 display means for state-year without and with RAI policies. Columns 4 and 5 display means for states with Republican and Democratic governors, respectively. Column 6 displays the p-value from a comparison of means between Republican and Democratic states.

Table 2-3. The effect of economic and social factors on the probability of RAI policy adoption

	(1) No interactions		(2) Full model	
Economic factors				
Percent federal revenue	-0.12	(0.07)	0.25	(0.24)
Percent tax revenue	-0.02	(0.02)	-0.02	(0.05)
Percent expenditures on corrections	0.88	(0.80)	0.18	(2.11)
Racial equity factors				
DI	0.03	(0.04)	0.32	(0.15)*
B/W incarceration rate ratio	0.11	(0.12)	0.34	(0.55)
B/W median income ratio	3.57	(1.98)	-5.10	(7.92)
Public safety factors				
Incarceration rate	0.00	(0.003)	-0.03	(0.01)*
Violent crime rate	0.00	(0.003)	0.02	(0.02)
Property crime rate	0.00	(0.001)	-0.001	(0.002)
Party				
Proportion Democratic legislature	-0.03	(0.03)	0.18	(0.23)
Party Interactions				
Federal rev*party			-0.01	(0.01)
Tax rev*party			-0.0004	(0.001)
Expenditures*party			0.02	(0.04)
DI*party			-0.01	(0.003)
B/W incarceration*party			-0.01	(0.01)
B/W median income*party			0.15	(0.14)
Incarceration rate*party			0.001	(0.0002)*
Violent crime*party			-0.0003	(0.0004)
Property crime*party			-0.00003	(0.00005)
Diffusion				
Percent neighbors with RAI policy	-4.38	(1.30)***	-6.80	(1.61)***
Division (comparison = New England)				
Mid- Atlantic	0.23	(1.82)	0.87	(1.84)
East North Central	-0.05	(1.90)	-0.25	(2.03)
North West Central	-0.74	(1.51)	-0.47	(1.67)
South Atlantic	-0.80	(2.37)	2.57	(2.16)
East South Central	0.16	(2.32)	3.25	(2.14)
West South Central	1.83	(1.94)	8.07	(2.84)**
Mountain	-2.52	(1.73)	0.09	(2.24)
Pacific	1.32	(1.38)	3.36	(1.71)*
State-level controls				
Proportion Black state residents	0.12	(0.08)	0.12	(0.06)
Unemployment rate	-0.20	(0.24)	0.01	(0.23)
Per capita income (logged)	-8.06	(3.38)*	-8.90	(4.02)*
Constant	83.72	(36.71)*	84.13	(42.92)*
N	403		403	
Wald	72.07***		108.37***	

Notes: Coefficients are log odds. Year fixed effects with robust standard errors in parentheses, clustered by state-year. ***p<0.001, **<0.01, *p≤0.05

2.6 Figures

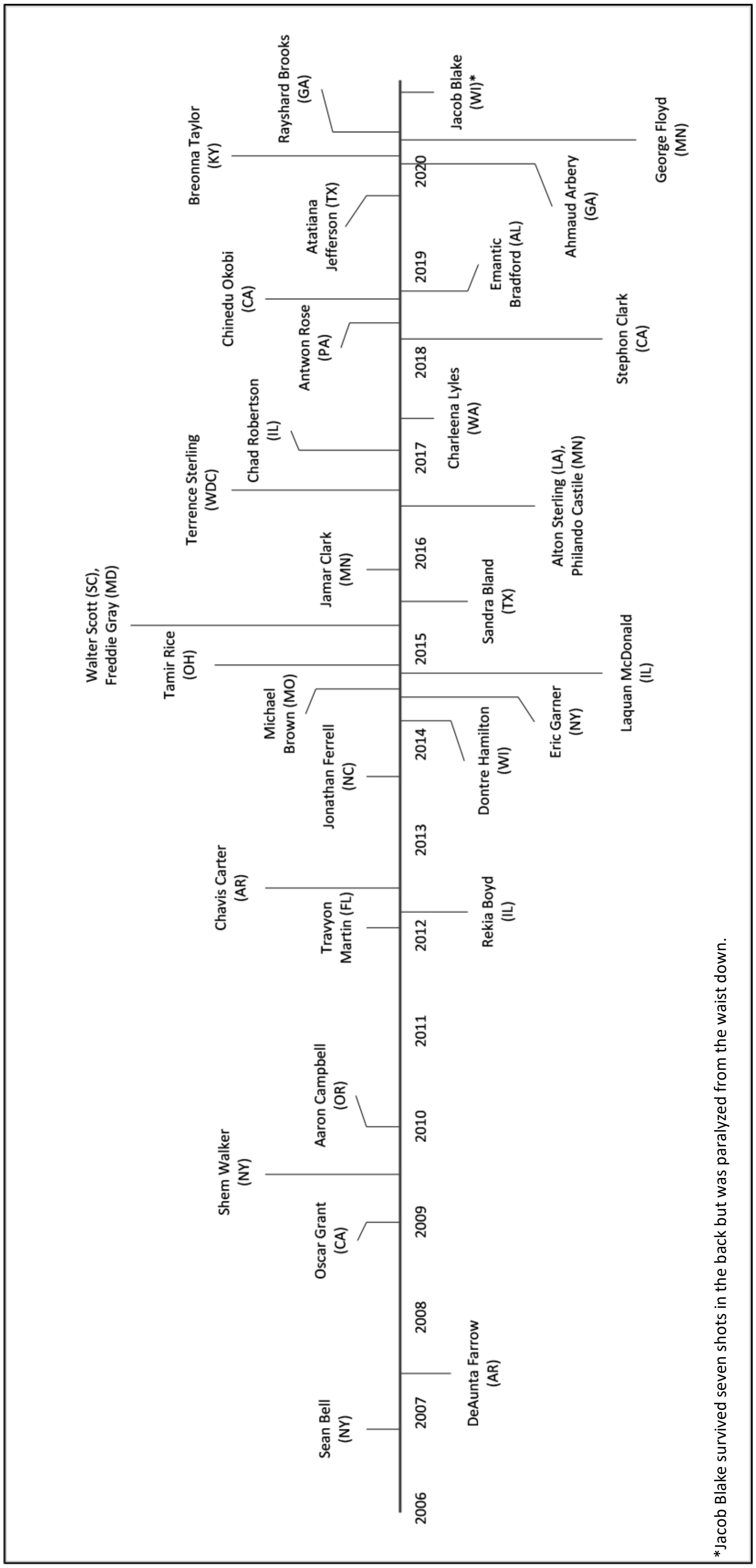


Figure 2-1. Timeline of high-profile deadly encounters with law enforcement

Figure 2-2. Cumulative percent of states with risk assessment policy

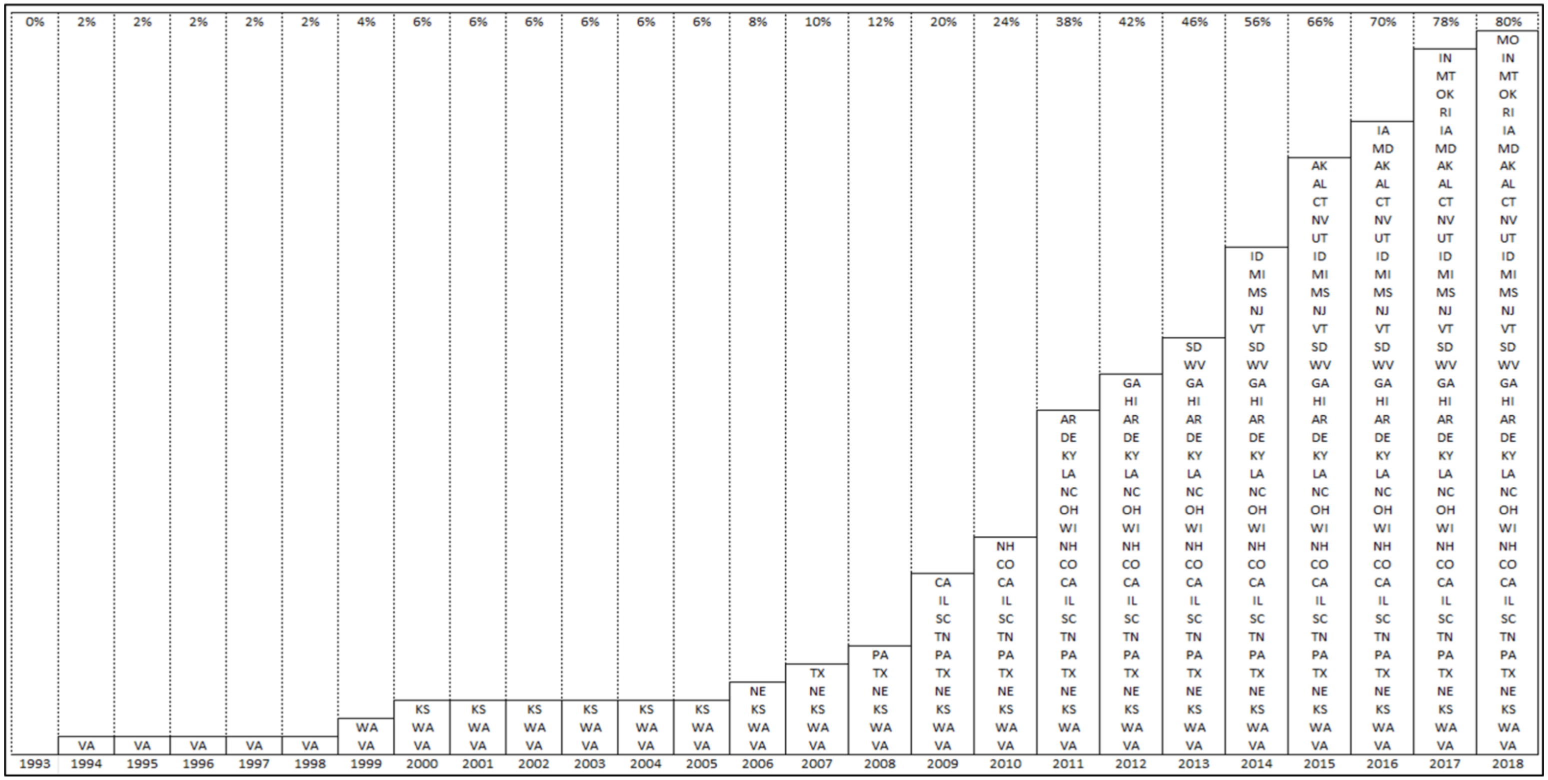


Figure 2-3 . Marginal effects of incarceration rate on state-level adoption as the proportion of Democratic legislators change

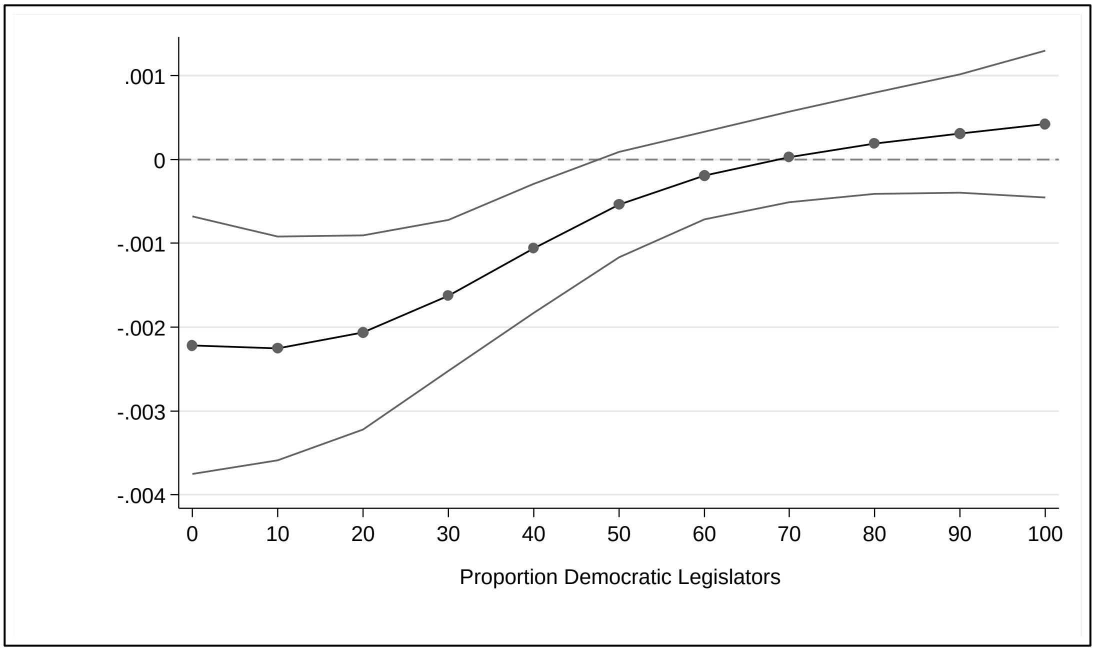
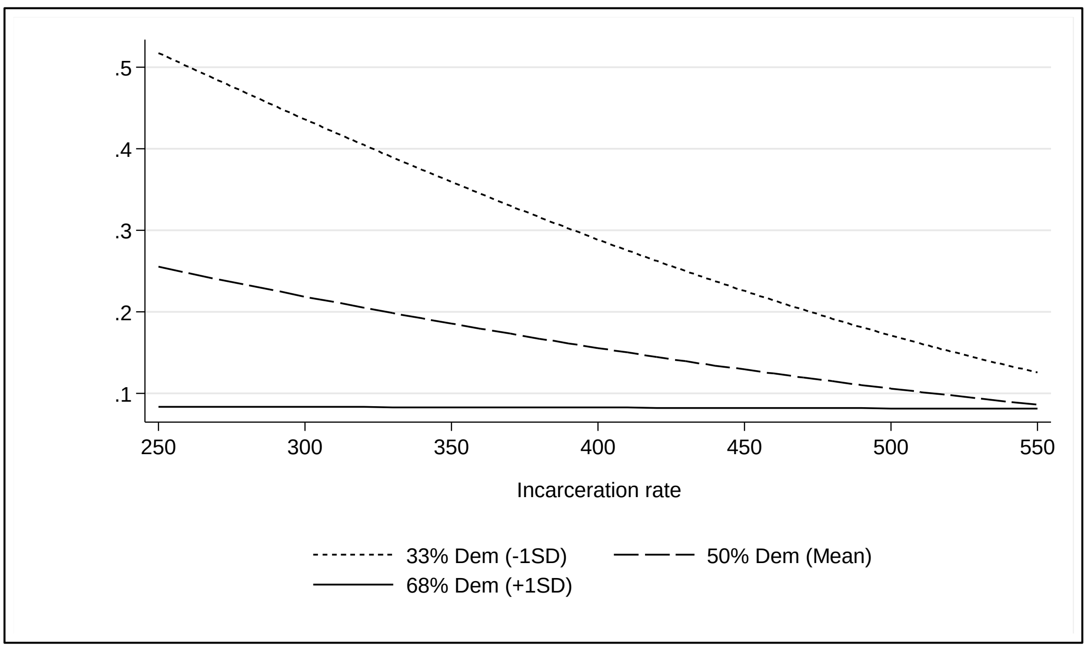


Figure 2-4. Predicted probability of adoption as incarceration rate changes by proportion of Democrats in the legislature



CHAPTER 3: THE EFFECT OF STATE-LEVEL RISK ASSESSMENT POLICY ON PRISON POPULATIONS

3.1 Introduction

In the criminal justice system, risk assessment instruments (RAIs) provide information that is used to make decisions about individuals at various points in the process. RAIs are expected to improve efficiency and fairness by quickly and easily identifying people who are most like other individuals who are noncompliant (e.g. fail to appear in court, or commit a crime) so that their behavior can be managed with the appropriate precaution or punishment. RAI policies are neither inherently punitive nor lenient, but as a process tool, RAIs have the potential to affect criminal justice outcomes. Four out of five states have passed laws mandating the use of RAIs in their criminal justice system. The first was passed in 1994 and the latest data available is from 2018. During that time the United States experienced an exponential rise in incarceration rates. An oft-cited reason for RAI policy adoption is to reduce the prison population. Yet it is unclear whether RAIs have been successful towards that end. This study asks 1) what were the effects of state-wide RAI mandates on prison populations, and 2) were there differences between early and late policy adopters?

In theory RAIs could lead to a reduction in prison populations in several ways. Generally, RAIs serve to move individuals away from a prison sentence. At pre-trial, assessments aid in the decision to hold or release individuals who have been arrested and detained. Studies show that those who are detained are more likely to be convicted and sent to prison (Dobbie, Goldin, & Yang, 2018; Lee, 2019; M. T. Stevenson, 2018). When RAIs identify someone as low risk, they are recommended for release. Likewise, RAIs at sentencing help identify low risk offenders who might do well in community supervision or treatment programs rather than prison. RAIs at parole serve a similar purpose but after an offender has already served part of their sentence in prison. If RAIs are indeed working as theory suggests, RAI policy should reduce overall imprisonment rates. It may take some time for effects to be detectable, therefor it is important to observe trends over time.

So far, RAIs appear to have mixed effects at a local level. The use of RAIs has led to declines in bail violation rates (Cooprider, 2009), and decreases in re-arrests for both violent and nonviolent crimes (Berk, 2017). However, other studies find they had no effect on the probability of incarceration or recidivism for nonviolent offenders (M. T. Stevenson & Doleac, 2019). These studies focus on the practice of using RAIs, with samples in a few jurisdictions. We have less understanding of how RAIs impact the system on a larger scale. A study of Kentucky's 2011 law mandating pre-trial RAIs found that the law changed bail setting practice, but had only small effects on pretrial release rates (M. Stevenson, 2018). Stevenson's research is one of the only studies to examine the effects of broader policy change regarding RAI adoption.

3.2 Methods

I use several difference in differences models to estimate the impact of RAI policy adoption on imprisonment rates and new prison admissions. I use an original state-level panel dataset of RAI policy adoption from 1990 to 2018 for the identification process. This period includes a few years before the first state-adopter through the most recent year with available data. Outcome measures were obtained from the National Prisoner Statistics via the Bureau of Justice Statistics Corrections Statistical Analysis Tool. Imprisonment rates are available for all 50 states and all 29 years yielding 1450 state-year observations. New prison admissions are available for the same periods, however, data are missing for 14 years in Alaska and 1 year in New Hampshire, leaving 1435 observations.

All analyses include the same covariates. They are measures associated with incarceration rates and RAI policy adoption, including the percent of the state population who are Black, the poverty rate, the percent of Democratic state legislators, and per capita income (logged) (see Chapter 2; Beckett & Western, 2001; Yates & Fording, 2005). Although research has shown there is little connection between violent and property crime rates and incarceration rates, crime rates are included in the model for good measure. Table 3-1 shows variable means with standard deviations in parentheses. Column 1 refers to the full panel of state-years. Columns 2 and 3 refer to state-years in states that did not adopt RAI policy during the study period and states that did, respectively. Columns 4 and 5 refer to state-year observations without and with RAI policy, respectively. States that passed RAI policies have higher means for both incarceration rate and new prison admissions than states that did not. However, Figure 3-1 shows the differences are consistent throughout the study period for both outcomes. On average there are only small differences in crime rates between states that did and did not have RAI policy by 2018 (columns 2 and 3); however, violent and property crime rates in untreated observations were much higher than in treated observations (columns 4 and 5). The remaining controls were similar across all columns.

The challenge in exploring policy effects is that all potential outcomes cannot be observed for the same unit after treatment, therefor we estimate the average causal effect. To estimate the effect of RAI policy adoption on outcomes I use difference in differences models. A simple difference in differences design subtracts trends in the pre-treatment period from trends in the post-treatment period for treatment and control groups to isolate the average treatment effect on the treated group (ATT). However, an additional hurdle arises when the policy is adopted at different times for different states. In that case, the pre- and post- treatment periods are muddled; some treatment group members become treated while others are treated later. A common approach to this type of study is the two-way fixed effect (TWFE) model. I begin by using the following regression:

$$Y_{i,t} = \alpha_t + \alpha_g + \beta_1 D_{i,t} + \beta_2 X_{i,t} + \varepsilon_{i,t}$$

where Y is the outcome for group i in time t, α_t is the year fixed effect, α_g is the state fixed effect, β_1 is the treatment effect for group i in time t, β_2 is a vector of state-level controls, and ε is the error. The year fixed effect accounts for time specific but group invariant unobserved confounders. The state fixed effect accounts for group specific but time invariant unobserved confounders.

However, Goodman-Bacon (2018) shows that TWFE estimates are sensitive to the size of each group, the timing of treatment, and the total number of time periods. The estimates in the TWFE model may be negatively weighed, returning a biased average if the treatment effect is heterogenous over time (Athey & Imbens, 2018; Borusyak & Jaravel, 2016; de Chaisemartin & D'Haultfœuille, 2020; Dettmann, Giebler, & Weyh, 2020; Goodman-Bacon, 2018). The difference in differences method developed by Calloway and Sant'Anna (2020) avoids the negative weights problem. Theirs is a staggered adoption model. In a staggered adoption, once the group is exposed to treatment, the treatment remains, i.e. the treatment cannot turn on and off. Education is the traditional example as knowledge does not disappear after the treatment has occurred, unlike some medications whose effects may wain immediately or shortly after they are stopped. My analysis includes forty state RAI policies adopted over more than twenty years. Heterogenous treatment effects are plausible because of the varying start dates and treatment durations (censored by the study period). For all non-TWFE estimations I use the Difference in Differences ("DID") and the Doubly Robust Difference in Differences ("DRDID") packages in R[11].

[11] The DRDID package is available on github at https://github.com/pedrohcgs/DRDID.

Callaway and Sant'Anna (2020) create cohorts by defining groups based on the time of first treatment and estimate group-time specific average treatment effects ($ATT_{g,t}$). Table 3-2 shows the cohorts in this study and the states included in each. Like the canonical difference in differences model, the main assumption is that there are parallel trends for treatment and control groups in the pre-treatment period. However, the group-time approach allows for conditioning on pre-treatment covariates where other approaches focus on unconditional difference in differences. I use outcome regression to model the conditional expectation of outcome trends for the comparison group using the same covariates as the TWFE model.

Another unique feature of the group-time approach is that it permits treatment group comparison to either never-treated or not-yet-treated groups. The decision is theoretical and based on the context of the specific study. Here, I use not-yet-treated groups for several reasons: 1) The not-yet-treated control yields a larger comparison group that includes both states which are never treated and states that are treated at a later time, which 2) makes that comparison group comparable to the control group in the other static and dynamic models, and 3) there is no reason to believe states that did not adopt RAI policy during the study period are fundamentally different than states that did[12]. I aggregate group-time averages by group to estimate cohort effects. To estimate the overall treatment effect, I average the effect for each cohort across all time periods. This estimation procedure yields results that are comparable to the ATT of the TWFE model.

In addition to cohort and overall treatment effects, I explore whether RAI policies had different effects depending on how long they were in place. Duration models, or event studies, center the data at the time of the event (adoption) and estimate effects of different exposure length,

[12] Results for never-treated control analyses are available upon request.

or dosage. My analysis includes the 9 years before and after the adoption of RAI policy. I aggregate the group-time averages by the number of years they experienced the treatment.

3.3 Results

For imprisonment rates and new prison admissions, the group-time model analyzes 16 cohorts over 28 years, yielding 448 group-time specific policy effects. Figure 3-2 shows group-time average effects of RAI policy adoption on imprisonment rates and 95% confidence intervals in each year for select groups. Cohorts 2009, 2011, 2014, 2015, and 2017 are the largest groups. Cohorts 1999, 2007, and 2008 have some of the strongest group-time effects. Pre-treatment trends are conditioned on state-level controls. Controls include states that were never treated during the study period or treated at a later time[13]. Robust standard errors are clustered by state. The vertical dashed line is the year of policy adoption for that cohort. Points before the line are pre-treatment effects, and points after the line are post-treatment effe cts. A visual inspection of the eight cohorts in Figure 3-2 reveals that most group-time effects preceding the dashed vertical line were around 0. In the full sample of observations, 90% of pre-treatment group-time average effects were null, providing evidence that the parallel trends assumption holds.

Post-treatment group-time effects are mixed. The graphs in Figure 3-2 show heterogeneity in the way cohorts responded to RAI policy. Cohorts 2007 and 2008 show immediate and statistically significant changes in the imprisonment rate. However, they are in opposite directions; the 2007 cohort showed a decrease in imprisonment rates, while the 2008 cohort showed an increase in imprisonment rates in response to policy adoption. 23% of the full sample post-treatment group-time effects were statistically significant. Of the statistically significant effects, 75% were negative. All statistically significant group-time effects were in small cohorts of just one or two states.

[13] A full table of group-time effects is available upon request.

It is difficult to make meaning of each group-time effect since there are so many groups and times. Using the aggregation method in the Doubly-Robust Difference in Differences package in R (Callaway & Sant'Anna, 2020), I averaged the group-time effects into cohort effects. Graph (a) in Figure 3-3 shows average effects of adopting RAI policy on imprisonment rates in each cohort across all their post-treatment periods. The 2000, 2007, and the 2018 cohorts saw imprisonment rates reduced by 30.10, 99.95, and 13.51 percentage points respectively. The 2008 cohort was the only group to experience a statistically significant increase (52.46 points) in imprisonment rates. Graph (b) shows policy effects on new prison admissions (logged). The RAI policy effect on prison admissions for the 2000 cohort was an 11.45% decrease. The 2010 cohort admissions dropped 10.79%. In contrast, the 2007 cohort admissions increased by 24.06% and the 2012 cohort admissions increased by 58.08%.

Next, I calculated the overall average treatment effects by aggregating the cohort effects. These are comparable to the traditional ATT recovered in the TWFE model. Table 3-3 shows the results for each outcome. Robust standard errors are in parentheses. The first column shows coefficients from the static two-way fixed effect model using state-level covariates, with state and year fixed effects. The second column (a) displays overall treatment effects from the group-time approach (Calloway & Sant'Anna, 2020) using never treated states as the control group, i.e., states that did not adopt RAI policy during the study period. The third column (b) shows overall treatment effects using the same method but using not-yet-treated states as the control group, i.e., states that did not adopt RAI policy or adopted RAI policy at a later time. Only policy effects on imprisonment rates were statistically significant. In the TWFE model the average treatment effect on the imprisonment rate was -20.54 ($p = 0.025$). The two estimates from the group-time approach were similar. I estimate a reduction in the imprisonment rate of 12.06 points using the never treated control group, and a reduction of 14.97 points using the not-yet-treated control group. However, only the latter is statistically significant. This is most likely due to the larger control group offering more efficiency and smaller standard errors.

The cohorts that were more strongly affected noticeably consist of states that adopted RAI policy early on. It follows that they also experienced the policy for a longer time. To test whether length of exposure to the policy influenced policy effects I use duration models from 9 years before to 9 years after policy adoption. Figure 3-4 shows the effect of policy adoption on a) imprisonment rates and b) new prison admissions (logged) for cohorts that had the policy for at least x years. Event times to the left of 0 are pre-treatment years. For both outcomes, pre-treatment effects are all very close to 0, again supporting the parallel trends assumption. Years to the right of 0, or post-treatment effects, are more varied but none are statistically significant.

3.4 Discussion and implications

Incarceration rates have begun to fall, but much more slowly than they rose, and there is uncertainty whether they will continue the downward trend or climb once more. There is an abundance of research on the rise of mass incarceration. It is just as important to study and understand the policies that are effective in decarceration. I find moderate evidence that mandating RAIs at the state level leads to a decrease in imprisonment rates but does not affect new admissions. I also find that RAI policy had stronger effects on early adopters, but not because they had the policy in place for longer. My results show neither that experiencing the policy for more years increases effects, nor that effects wain over time. However, the ATTs hide group and group-time effects that vary widely by cohort and provide a wealth of information about when and where the policy was effective.

Group-time and cohort effects are a good starting point for exploring effective assessment policies for prison population reduction. As with many difference in differences models, averages appear highly influenced by group context and the size of the group. Fortunately, using Calloway and Sant'Anna's group-time approach we can see where those differences are. By estimating policy effects for every cohort in each year of the study, these results provide a guide for further research on when and where RAI mandates had a significant effect on prison populations. Policy effects on imprisonment rates were strongest on early adopters and smaller cohorts. For example, the average treatment effect of RAI policy adoption on the 1999 cohort was an 81.54 point reduction in the imprisonment rate. The 1999 cohort consists of only one state, Washington, and all but three of its group-time effects were null. Examining their group-time effects over time shows a large drop in the imprisonment rate from 2014 to 2015. It is unlikely that the RAI policy would cause a sudden drop six years after it was adopted. The policy effect is probably spurious. In fact, Washington experienced a large increase in the percentage of inmates released that year (Carson & Andersom, 2016), which might explain the change. All but one of the statistically significant post-treatment group-time effects came from cohorts with only one state, and 86% were from just three states: Kansas, Texas, and Pennsylvania. A case study on these three states might reveal the necessary factors for strong policy outcomes, perhaps in some combination with other policy reforms.

Like imprisonment rates, the cohort and group-time effects for new admissions were diverse. 20% of the post-treatment group-time effects were statistically significant and spanned 7 cohorts. Cohort effects were quite strong, with one group seeing an increase in new prison admissions of almost 60%. These cohorts were also small, consisting of one or two states. At times, the effect on admissions is in the opposite direction as the imprisonment rate. In other words, new prison admissions increased while imprisonment rates decreased, and vis versa. This is likely due to heterogeneity in RAI policy design across states. Policies mandate the use of RAIs at different points in the process which has implications for the relationship between policy and outcome. For example, I would expect pretrial RAIs to have direct effects on jail populations, but indirect effects on prison populations since that is a few steps removed from bail decisions. The number and combination of policy components may also influence outcomes. For example, some states use a package of assessments at multiple points in the process. Future analyses should evaluate whether a system of assessments is more effective than any one RAI on its own.

More information about meso-level policy implementation is needed. Averages on a macro-level, even using innovative methods, wash out policy heterogeneity. On the ground level, there is a rich literature building around how actors use, or misuse, RAIs as tools for decision-making and how that affects different groups of individuals (Corbett-Davies, Pierson, Feller, Goel, & Huq, 2017; Kleinberg, Lakkaraju, Leskovec, Ludwig, & Mullainathan, 2017; Skeem, Scurich, & Monahan, 2019; M. T. Stevenson & Doleac, 2019). However, to understand why states respond so differently to RAI policies, we need more information about how states and local jurisdictions choose to fulfill policy mandates. For example, the choice of assessment may have different effects on different jurisdictions even when used at the same point in the process. Another important factor may be the position of the individual filling out the form; The responsibility of filling out the assessment falls to different court officials and is sometimes partially automated. Procedural characteristics such as these are often codified and can be included in empirical analysis. These data can be combined with implementation data to uncover the mechanism of policy effectiveness.

3.5 Tables

Table 3-1. Descriptive statistics of model covariates

Variable	(1) Full Sample	(2) No RAI policy	(3) RAI policy	(4) Untreated Observation	(5) Treated Observation
Imprisonment rate	391.32	294.47	415.5414	383.57	421.19
	(161.60)	(140.81)	(157.39)	(166.32)	(138.24)
New prison admissions	7571.98	6867.13	7750.50	7065.35	9496.83
	(9102.99)	(9247.27)	(9061.46)	(8624.78)	(10523.07)
Percent Black population	10.83	5.95	12.04	10.25	13.05
	(9.59)	(6.01)	(9.93)	(0.54)	(9.50)
Violent crime rate	433.74	431.13	434.39	450.74	368.29
	(213.57)	(263.79)	(199.18)	(227.77)	(127.14)
Property crime rate	3456.62	3482.51	3450.151	3642.162	2742.39
	(1110.88)	(1392.87)	(1029.01)	(1115.28)	(745.42)
Poverty rate	12.69	13.10	12.59	12.68	12.76
	(3.54)	(3.45)	(3.56)	(3.57)	(3.43)
Percent Democrats on state	49.84	49.87	49.83	51.75	42.46
legislature	(17.77)	(16.81)	(18.01)	(17.39)	(17.30)
Per capita income	43532.22	44719.43	43235.42	42169.51	48777.97
(2018 dollars)	(8389.19)	(9013.98)	(8202.87)	(8232.67)	(6772.86)
Number of observations					
Imprisonment rate	1450	290	1160	1151	299
New prison admissions	1435	290	1145	1136	299

Note: Standard deviations in parentheses. Column one refers to the full panel of state-years. Columns 2 and 3 refer to state-years in states that did not adopt RAI policy during the study period and states that did, respectively. Columns 4 and 5 refer to state-year observations without and with RAI policy, respectively.

Table 3-2. State cohort assignments

Group/Cohort	States
1994	VA
1999	WA
2000	KS
2006	NE
2007	TX
2008	PA
2009	CA, IL, SC, TN
2010	CO, NH
2011	AR, DE, KY, LA, NC, OH, WI
2012	HI, GA
2013	SD, WV
2014	ID, MI, MS, NJ, VT
2015	AK, AL, CT, NV, UT
2016	IA, MD
2017	IN, MT, OK, RI
2018	MO

Table 3-3. Comparisons of average treatment effects on the treated

		Aggregated Overall Treatment Effect	
Outcome	TWFE	(a) Never treated	(b) Not Yet Treated
Imprisonment rate	-20.54	-12.06	-14.97
	(8.91)*	(6.17)	(6.68)*
New prison admissions (logged)	-0.00005	0.07	0.05
	(0.05)	(0.05)	(0.04)

Note: Robust standard errors in parentheses. *p< 0.05. All models are clustered at the state level. The first column shows coefficients from the static two-way fixed effect model using state-level covariates, and state and year fixed effects. The second column displays overall treatment effects from the group-time approach (Calloway & Sant'Anna, 2020) using never treated states as the control group, i.e. states that did not adopt RAI policy during the study period. Column 3 shows overall treatment effects using the same method, but using not yet treated states as the control group, i.e. states that did not adopt RAI policy or adopted RAI policy at a later time. These two models use state-level pre-treatment covariates.

3.6 Figures

Figure 3-1. Imprisonment rates for states that adopted RAI policy and states that did not

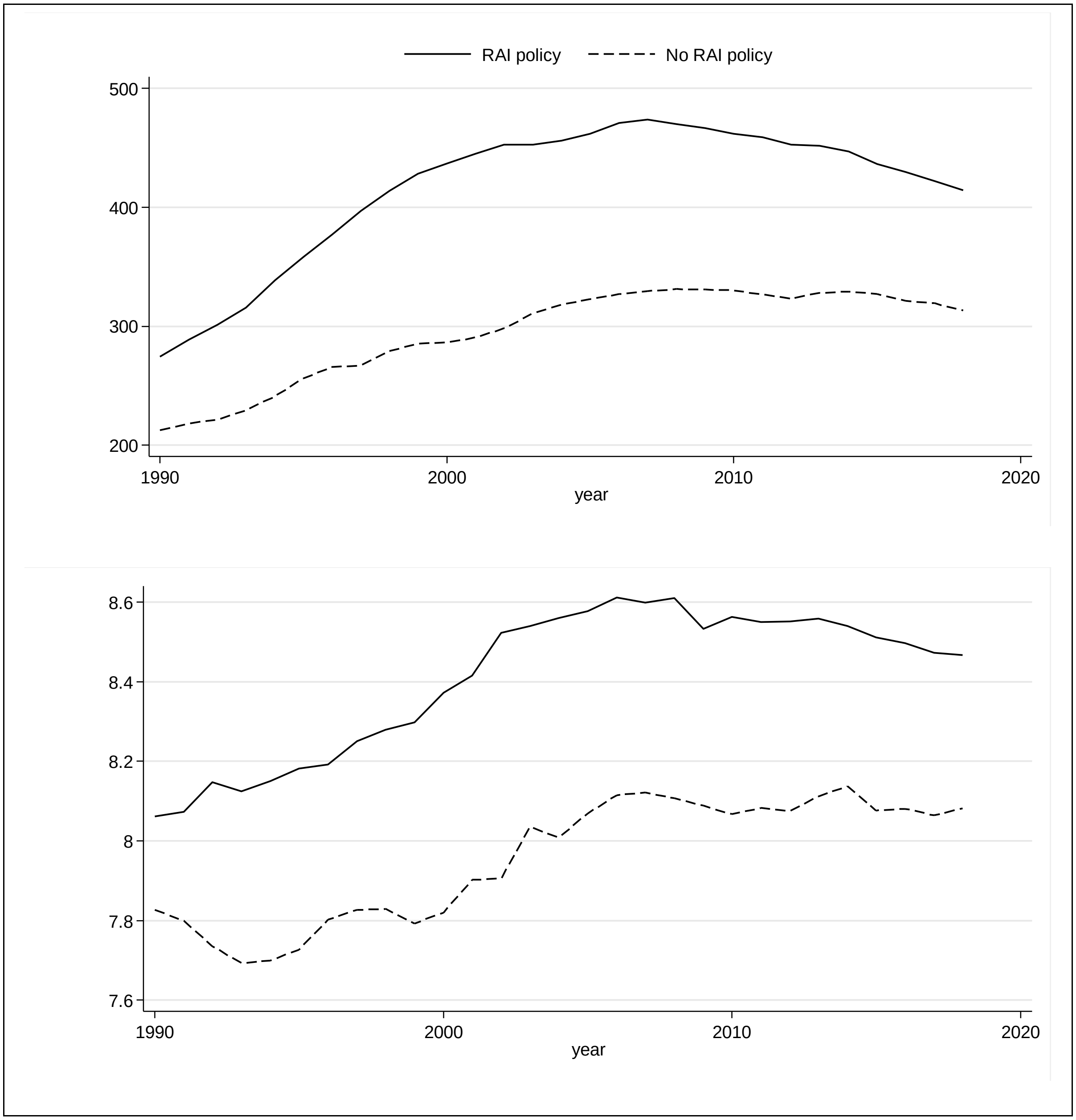


Figure 3-2. Average group-time treatment effects for select cohorts

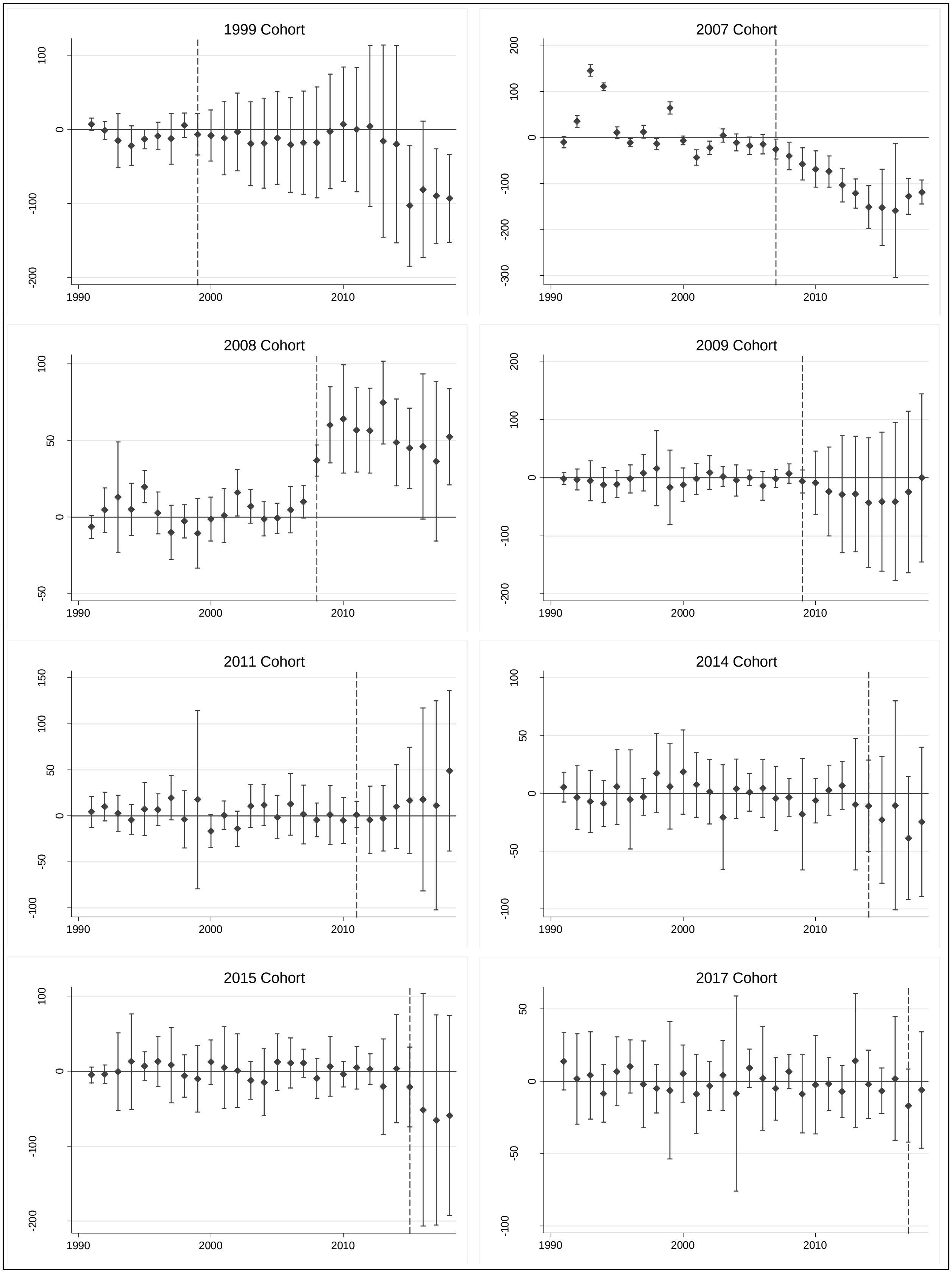


Figure 3-3. Average cohort treatment effects on imprisonment rate and new prison admissions

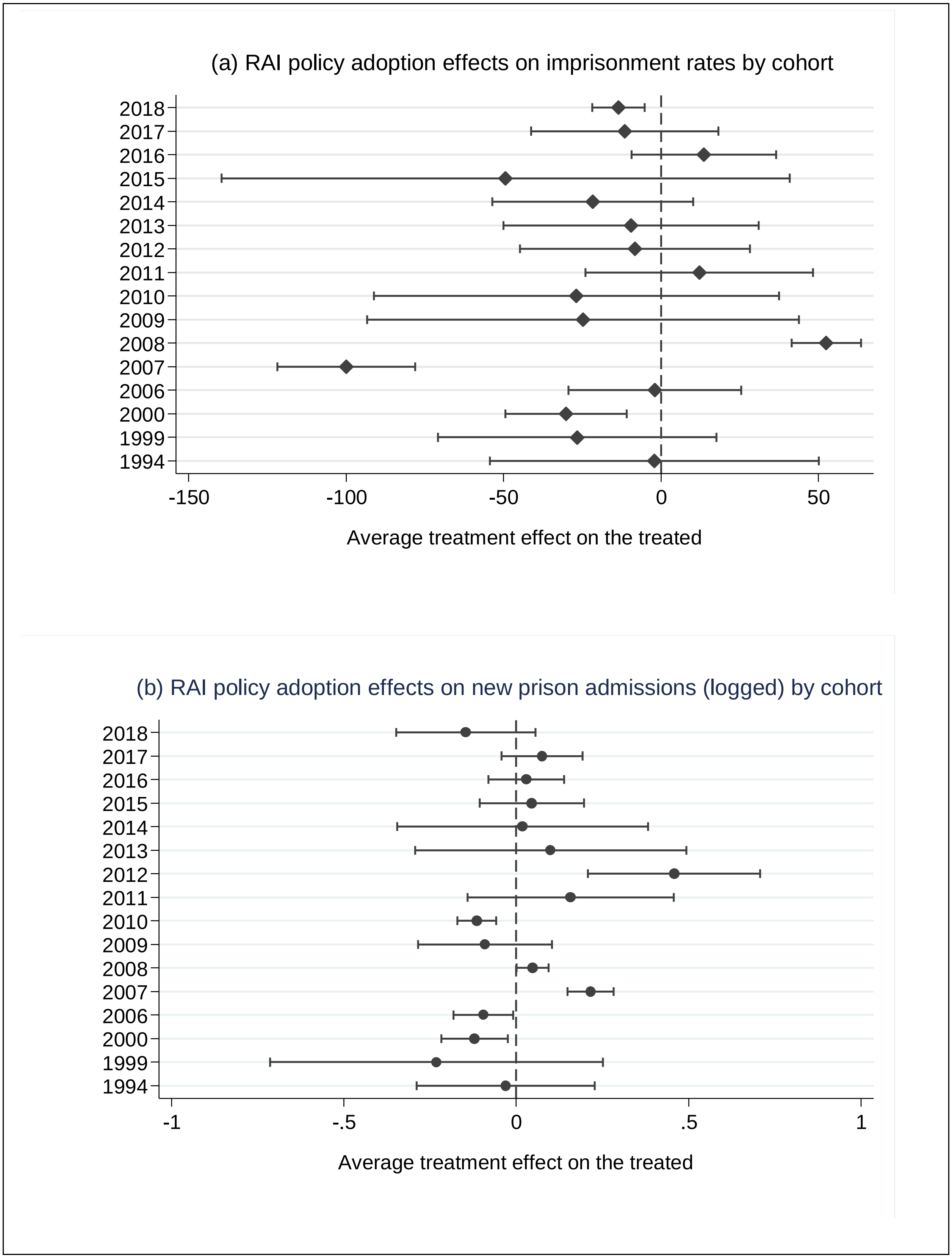
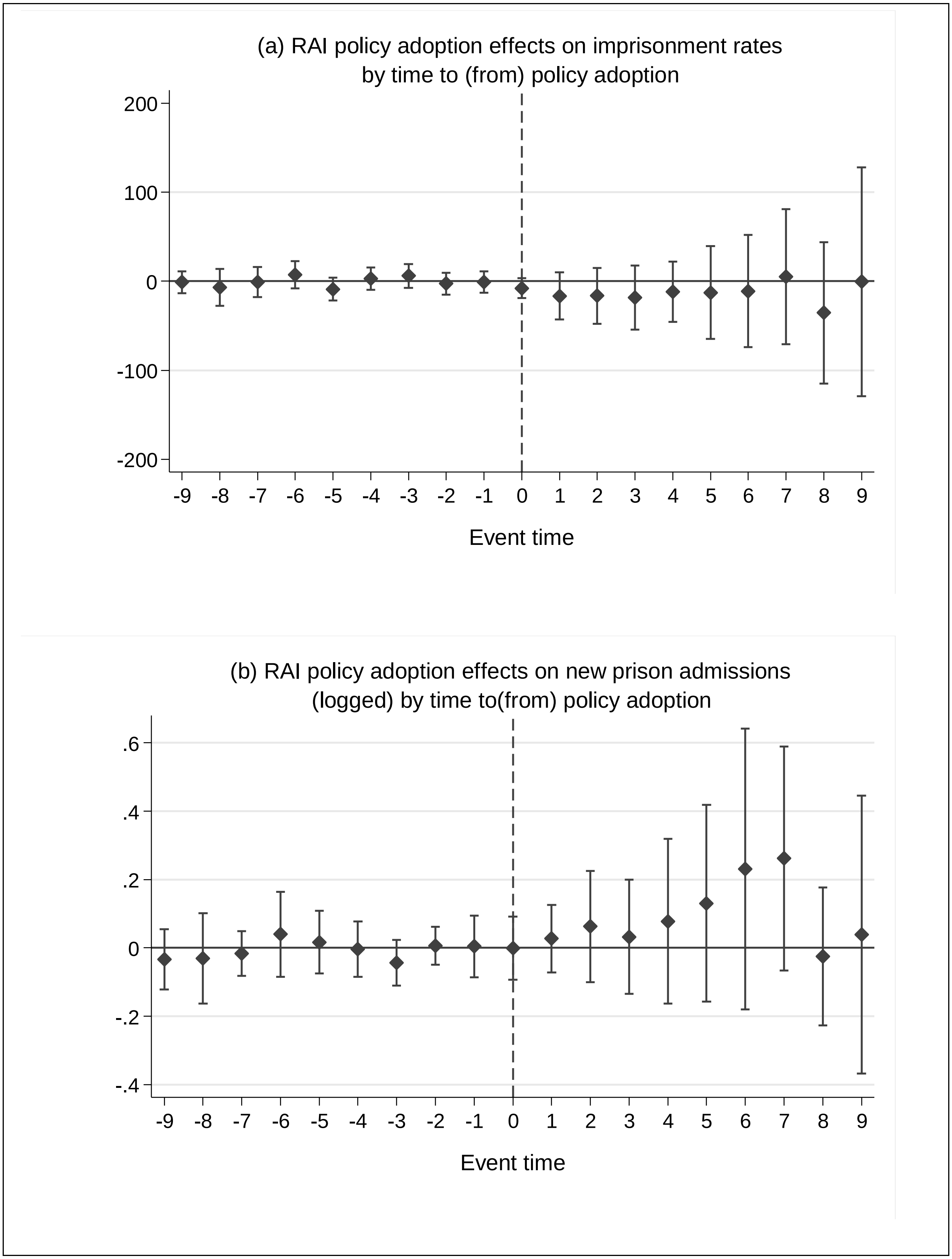


Figure 3-4. Average treatment effects on the treated by years of exposure



CHAPTER 4: THE EFFECT OF STATE-LEVEL RISK ASSESSMENT POLICY ON RACIAL DISPARITIES IN PRISON SENTENCE LENGTHS

4.1 Introduction

In the late 2000s risk assessment instruments (RAIs) gained prominence as a promising tool to reduce prison populations and racial disparities throughout the criminal justice system. RAIs are commonly used at multiple points in the criminal justice system, but are still controversial, especially at sentencing. In 2007, the Conference of Chief Justices and the Conference of State Court Administrators enacted resolutions calling for the use of best practices to reduce recidivism, naming risk assessments one of those best practices. A working group of court leaders, practitioners, and researchers published a report with guidelines on the use of risk assessments at sentencing. The National Conference of State Legislatures published a companion piece with Pew Research detailing how 10 jurisdictions use the assessments and how well they align with the guidelines. However, a supreme court case in Wisconsin ruled that "using a risk assessment tool to determine the length and severity of a sentence is a poor fit" (Wisconsin, 2016). Nevertheless, by 2018 eight states had passed legislation mandating the use of RAIs at sentencing for some offenders. These actions were guided by hope more than evidence. Though there is an ongoing and intense effort to validate assessments for accuracy and equity, we know little about the broader impacts of policies that force the courts' hand regardless of local environment or the specific brand of risk assessment. This study examines whether imposing a state-wide mandate to use RAIs for sentencing decisions results in equitable outcomes.

the harsh treatment of Black individuals by law enforcement, courts, and corrections systems is well documented. The Sentencing Project estimates that African Americans are incarcerated at 5.1 times the rate of White Americans, with some states reaching a ratio of more than 10 to 1 (Nellis, 2016). The causes of systemic racial disparities in prisons and jails are structural, stemming from legislation and local policies that have disparate impacts, unintended or otherwise (Alexander, 2010; Mauer, 1999; Mesic et al., 2018; Roberts, 2004). The high proportion of individuals, mostly men, in Black communities who leave for and return from prison has caused economic and social loss that will be felt for generations (Clear, 2007; Roberts, 2004; Western & Wildeman, 2009). In the 1990s there was a movement towards a more prescriptive system which limited judicial discretion and bias (Tonry, 1995). However, the laws that emerged did little more than fill prisons and devastate entire communities. Federal and state policies like determinant sentencing, mandatory minimums, and Three-Strikes laws, are now associated with mass incarceration rather than public safety. Risk assessment policies fall into a middle ground; they require court officials to consider assessment results but stop short of mandating any particular sentence based on those results.

Research on sentencing practices may include two outcomes of interest: 1) the decision to send convicted offenders to prison or to alternative programing, and 2) sentence length. Risk assessments may be used to support decision-making for both sentencing outcomes. Studies using data especially from the 1980s and 90s find that race plays a role in the former but not the latter (Chiricos & Crawford, 1995; Spohn, 2000). However, more recent work has found racial differences in sentence length (Doerner & Demuth, 2009; Franklin, 2018). I estimate the change in racial inequality in sentence length when RAI policy is imposed.

RAI sentencing policies mandate an assessment of risk for some sentencing decisions. They are used in conjunction with sentencing guidelines and judicial discretion (Elek, Warren, & Casey, 2015). High-risk scores suggest harsher penalties. The bulk of RAI literature consists of validation studies focused on the merits and mechanics of individual assessments. Overall, the instruments are more consistent and accurate than human discretion, even professional ones (Goel, Shroff, Skeem, & Slobogin, 2018; Jung, Concannon, Shroff, Goel, & Goldstein, 2017). Validation analysis usually includes a comparison of false positive rates for different races, called classification parity. Some assessments also undergo calibration to ensure that outcomes are independent of protected attributes, like race, conditional on risk score (Goel et al., 2018). However, scholars have also noted that different measures of fairness are impossible to hold at once (Berk, Heidari, Jabbari, Kearns, & Roth, 2018; Corbett-Davies & Goel, 2018) and are sometimes counterproductive to avoiding disparate impacts (Goel et al., 2018). Risk assessments are included in a larger conversation about automated racism. Researchers have found that because algorithms are built using biased historical data, they disproportionately target and harm people of color (Eubanks, 2018; Noble, 2018; O'Neil, 2017). There is concern that RAIs could and will be used as tools of social control (Silver & Miller, 2016).

This scholarly debate over racial bias in risk assessments is ongoing, all the while local jurisdictions and states continue to adopt policies that turn the practice of using RAI scores into law. There is much less attention on the effects of those policies. To ensure that risk assessments are equitable it is important to understand not just the instrument itself but how outcomes play out after they are put in the hands of people, and on a large scale. Another benefit of broadening analysis to policy rather than instrument is that it allows for the inclusion of more jurisdictions. I use data on all 50 states with the goal of developing a sense of what states can expect regarding racial disparities as a result of legislating RAIs at sentencing.

4.2 Methods

I used offender-level data from the National Corrections Reporting Program (NCRP) Prison Admissions 1991-2016 with selected variables dataset (US Department of Justice, 2018). NCRP data include offender's race, age range, education, state of jurisdiction, year of admission, offense type, and sentence length. All variables, including the dependent variable, are categorical. I used only new prison admissions to capture the effect of newly implemented policy and because analysis of general incarceration rates overrepresent offenders serving long sentences (see Felson & Krajewski, 2020). I dropped cases of admissions for parole revocation, cases of non- White or Black offenders, and cases that were missing sentence length, admission year, race, or offense type leaving a final dataset of 3,408,064 observations across 46 states. Table 4-1 shows the proportion of offenders in each category for all covariates. About 56% of inmates were White, and 44% were Black. Black inmates were more likely to be imprisoned for violent and drug related crimes than White inmates, and White inmates were more likely than Black inmates to be in prison for property crimes. Sentence length, the dependent variable, is recorded in 7 levels: 1) less than one year, 2) 1-1.9 years, 3) 2-4.9 years, 4) 5-9.9 years 5) 10-24.9 years 6) 25 years or more, and 7) Life in prison, life in prison without parole, or death. The most common sentence length for all inmates was 2-4.9 years. The within race proportion of offenders at each sentence level was similar for Black and White offenders.

For policy data I used an original panel dataset of state-wide RAI policies from 1990 to 2018. I coded each state-year observation with indicators for several policy components in years when RAI policy was present, including an indicator when the law requires RAIs for sentencing decisions for some offenders. A second coder did the same. Indicator assignment matched for 92% of coding decisions (Cohen's kappa = 0.74). I then merged offender admission data with state policy data from the previous year to allow time for the policy to take effect. In 2018, eight states mandated the use of RAIs at sentencing for particular offenders[14]. In some cases, the law is unclear

[14] The states which have RAI sentencing policies are Kansas, Kentucky, Louisiana, Montana, Ohio, Tennessee, Utah, and Virginia.

on which offenders should receive an assessment. For example, the Montana Code Annotated stipulates that a defendant's risk assessment results shall be reported "whenever a [presentence] investigation is required" (MCA). However, the aim of this research is not to test effects of the assessments themselves, but rather to estimate the effect of the policy, including the design choice whether or not to specify clearly which sentencing decisions should consider RAI results. Therefor policy effects are valid regardless of the prevalence of actual RAI score consideration. It would be plausible and interesting to estimate the effects on a target population, however, given that most policies do not provide a target population, results from this analysis should be interpreted broadly as average effects of RAI sentencing policy for all offenders sentenced to state prisons.

The nature of sentencing level is ordinal, from shortest/least severe to longest/most severe. Therefor to estimate the effect of RAI sentencing policy on sentence length for Black and White inmates I used a series of ordered logistic regressions. Ordered logistic regressions provide odds ratios of receiving a longer sentence and allow for predicted probabilities under specified conditions. I used offender age, gender, and education as controls. Robust standard errors were clustered by state to account for state-level trends including other policies. Estimates are reported for all offenses combined, and separately for each offense type.

Ordered logistic regressions assume proportional odds, i.e. that covariates have the same effect across outcomes. For example, it assumes that the odds of a Black offender receiving a sentence of less than one year vs. any other sentence level are the same as the odds of a Black offender receiving a sentence of 1-4.9 years vs. any other sentence. However, the dataset is large with over 3 million cases. Tests of proportional odds, or parallel regression, nearly always fail when the sample size is large (Allison, 1999; Clogg & Shihadeh, 1994). To observe if and to what degree the assumption is violated I ran a series of binary logistic regressions for each sentence outcome using one covariate at a time. Table 4-2 shows that the odds of being sentenced to different levels varies for Black offenders. For example, for property crimes the odds of a Black offender receiving a sentence of life in prison or death is 2.24 times that of White offenders, while the odds for a sentence of 5-9.9 years is .84 that of White offenders. This confirms violation of the proportional odds assumption. However, it also confirms the racial disparities in sentencing length found in recent research. This paper asks whether RAI sentencing policies moderate the "effect of race" [15]. If they do, the effect of race should not be statistically significant in jurisdictions with RAI policies.

As a robustness check I also estimated effects using multinomial logistic regressions for each offense category with the same specifications. This analysis returns separate estimates for the odds of receiving each sentence level. Multinomial logistic regressions assume independence of irrelevant alternatives, meaning the ratio of any two probabilities are independent of the remaining choices. In this case, it assumes that if any sentence level were omitted, offenders would be sorted into the remaining levels proportionally. This is usually a strong assumption, however, here the assumption holds. Offenders do not self-select their sentence level. It is reasonable to presume that offenders sentenced to the omitted category would instead be sentenced to an alternative of equal severity, and the remaining offenders would still be sorted into the appropriate sentence levels.

I also ask whether RAIs reduce racial differences in the probability of receiving a given sentence length. It is possible for a policy to moderate the effect of race such that the gap between races is widened, or affects one race more than the other. To probe further, I compare the difference in predicted probabilities between races with and without the imposition of RAI policy.

[15] To be clear, race itself does not cause outcomes. Instead, many cultural and historical factors, some observable and some not, are highly correlated with race so that the variable "race" captures the average effect of the experience of being of a particular race.

4.3 Results

Results from the ordered and the multinomial logistic regressions were similar, with ordered results slightly more conservative[16]. A test of Akaike's Information Criteria returned the same score for both models. Here, I discuss the results of the ordered logistic regression because it allows for comparative language of "longer" and "shorter" sentences, as opposed to speaking of each sentence level separate from the others. There is one model that includes all offenses, and separate models for each offense type. The independent variables of interest in all models are 1) race, 2) an indicator for the presence of RAI sentencing policy at the time of admission, and 3) an interaction between the two. Offender age, education level, and gender, were controls, and robust standard errors were clustered by state. Table 4-3 shows the odds ratios resulting from the regressions. Holding all else constant, the only factor with a statistically significant effect on sentence length was race in cases of violent crime. The odds of Black inmates receiving a longer sentence for a violent crime was 1.17 times that of White inmates, confirming racial bias in violent crime sentencing outcomes. The interactions between race and policy were not statistically significant suggesting that RAI sentencing policies have no effect on the odds of Black inmates receiving longer sentences compared to White inmates without the policy.

However, the interaction of odds ratios are difficult to translate into real world effects. To make direct comparisons between Black and White offender sentences I calculated the difference in predicted probabilities for both races for each type of crime at every sentence level. Full tables of predictions are available in Appendix B. Figure 4-1 displays the predicted probabilities of each sentence level for Black offenders relative to White offenders and 95% confidence intervals with and without RAI sentencing policy. Open circles show the difference between Black and White offender probabilities of receiving x sentence length when there is no RAI sentencing policy in place

[16] I also ran split models removing the interaction between policy and race. The estimates were similar.

at the time of admission. Solid circles show the difference when there is RAI policy at the time of admission. Graph (a) confirms that there is little difference between races in sentence length across all offenses with all points hovering around zero. Graphs by offense type, however, tell a different story. For violent crime, graph (b), without a RAI policy Black offenders have a statistically significant lower probability than White offenders of receiving a sentence of less than 5 years, but a higher probability of receiving a sentence of more than 10 years or the death penalty. The largest gap is at 10-24.9 years where Black offenders are 2.34 percentage points more likely than White offenders to receive a sentence within that range. In contrast, with RAI policy all confidence intervals cross 0. This suggests RAI sentencing policy reduces racial disparities in sentencing offenders who have committed violent crimes.

For property crimes and drug crimes, estimated differences without RAI policy are larger than violent crimes but not statistically significant. Like violent crimes, racial disparities are closer to zero and not statistically significant with RAI policy. Racial differences without RAI policy in public order crimes are the smallest of all offense types. It is also the only offense type to show increases in disparities with RAI policy. Black offenders are more likely than White offenders to receive a sentence of less than one year, and less likely to receive a sentence of 5-9.9 years with a policy. RAI policy seems to increase racial differences in public order crimes at shorter sentences. Public order crimes include prostitution, pornography, and driving under the influence.

I also calculated within race differences with and without RAI sentencing policy. There are no statistically significant differences for either race. However, results illustrate potential benefits to both Black and White offenders. Taking violent crime as the example, Figure 4-2 shows the change in predicted probability of sentence length for both races holding all else constant. Each graph shows the difference for the given sentence level. Solid circles are predictions for Black offenders, and triangles are predictions for White offenders. The left half of each graph is the probability when no one has a RAI policy at the time of admission, and the right half is the probability when everyone has a RAI policy. The dashed line connects points with and without a policy for each race, with the difference in text. When a RAI policy is imposed the probability of being sentenced to less than 5 years increases for both races (graphs a-c). The probability of being sentenced to more than 5 years or death decreases for both races (graphs d-f). This is evidence that the decrease in racial disparities with RAI policy is not due to subjecting White offenders to harsher penalties in lieu of Black offenders, but rather due to differences in the magnitude of change for each race.

4.4 Discussion and implications

I find no evidence that RAI sentencing policies alter the effect of race on sentence lengths. The odds of receiving longer or shorter sentences do not change for Black or White offenders when the state adopts a RAI policy. However, there is some evidence that RAI policy narrows racial disparities in certain contexts. There are no policy effects when all offenses are analyzed together, however, patterns emerge by offense type. The policy effect is especially pronounced in cases of violent crime. Imposing RAI policy eliminates Black and White offender differences in the probability of receiving a sentence of less than 5 years and more than 10 years. Though the percent point changes are small, they imply large real world effects. If all offenders were in jurisdictions with RAI sentencing policy at the time of admission an estimated 25,701 Black offenders and 37,664 White offenders would have avoided a lengthy sentence of 10-25 years for violent, property, and drug crimes.

The direction of the Black/White disparity differs depending on sentence length. Black offenders are more likely than White offenders to receive long sentences for violent crime, and less likely than White offenders to receive shorter sentences. However, there are smaller or no differences in most other offense categories. These results support literature on "contextual discrimination" which finds that racial minorities are sentenced more harshly in some jurisdictions in some circumstances (Spohn & Delone, 2000; Walker, Spohn, & DeLone, 2016). This is likely because the nature of racial stereotypes is also complex. Black individuals are judged as less deserving, more aggressive, or held more responsible than other groups depending on the circumstances (Holbrook, Fessler, & Navarrete, 2016; Howard, 2019; Kreitzer & Smith, 2018; Reyna, Henry, Korfmacher, & Tucker, 2006; Tonry, 2010). Therefor the context of crime is of great importance. Information about the environment of the jurisdiction, – such as political and punitive preferences – details about each case, – such as the race of the victim or the arresting officer or the judge, or about the crime within the offense category – and about the offender – such as criminal history and unemployment status – would allow for more precise models and shed light on when and why policy affects equitable outcomes.

Nevertheless, these results demonstrate differential policy effects by offense type and provide a starting point for future investigation of the impact of RAI policy in multiple fields. One of the main foci in criminology and sociology literature is on disparate impacts of drug policies and court practices in drug cases. My results provide little evidence that RAI sentencing policies make a significant impact on racial disparities in drug crimes. However, there is strong evidence that RAI policies reduce racial disparities in sentencing for violent crimes. This is an important result to note. These results run counter to previous studies which have found larger racial differences in sentencing for non-violent and less serious crime. The rationalization seems to be that White offenders are not given the same amount of leniency in serious crimes so racial differences are minimized. Yet, research on capital punishment (the most serious penalty reserved for the most heinous crimes) is clear that Black offenders have been unfairly targeted and sent to death row (Baumgartner et al., 2018; Kotch & Mosteller, 2010; Steiker & Steiker, 2015). My results are more consistent with research on race, structural inequality, and general incarceration rates (M. T.

Stevenson & Doleac, 2019). It would be far more surprising if racial inequality existed for crimes at low and high levels of severity, but not in between.

In addition, there has been movement to address historical injustices in sentencing for drug crimes. The First Step Act of 2010 reduced mandatory sentence lengths for crack cocaine crimes closer to those of powder cocaine in the federal prison system. This resulted in over 3,000 sentence reductions and over 2,100 releases (Justice, 2020). States have passed similar laws often in a "Justice Reinvestment" act. However, reform efforts have not focused on violent crime. In fact, criminal codes have gotten stricter regarding violent crime. Research shows that liberal state governments are especially prone to reducing the severity of punishment for non-violent and drug crimes while increasing penalties for violent crimes (Barker, 2009). This is a politically palatable compromise for both politicians and the public. However, it may exacerbate racial disparities in the prison system as a larger proportion of inmates are there for more serious crimes. Unless disparities for those offenses also receive attention, policymakers will have to decide whether they are willing to sacrifice justice for more serious crimes in return for addressing injustice at lower levels. These results suggest that mandating RAIs at sentencing may increase racial equity in sentencing practices while also shortening prison terms for offenders of all races.

4.5 Tables

Table 4-1. Proportion of offenders at each covariate level

	Level	(1) All	(2) Black	(3) White
N		3408064	44.1%	55.9%
Policy	No RAI	91.2%	92.5%	90.1%
	RAI	8.8%	7.5%	9.9%
Sentence length	< 1 year	18.4%	18.7%	18.2%
	1-1.9 years	10.0%	9.3%	10.6%
	2-4.9 years	36.1%	36.1%	36.1%
	5-9.9 years	20.9%	20.7%	21.1%
	10-24.9 years	11.7%	11.8%	11.6%
	>=25 years	1.9%	2.2%	1.6%
	Life/Death	1.0%	1.2%	0.8%
Highest level of education	<HS diploma/GED	31.0%	35.9%	27.1%
	HS diploma/GED	29.3%	27.3%	30.9%
	Any college	6.0%	5.2%	6.7%
	Ungraded/unknown	33.7%	31.6%	35.3%
Gender	Male	85.7%	90.6%	81.9%
	Female	14.3%	9.4%	18.1%
Age at admission	18-24 years	24.1%	29.3%	20.0%
	25-34 years	34.2%	32.6%	35.4%
	35-44 years	23.2%	20.7%	25.2%
	45-54 years	14.3%	13.6%	14.9%
	55+ years	4.1%	3.1%	4.4%
	Missing	<1%	<1%	<1%
Categorization of most serious sentenced offense	Violent	24.8%	28.2%	22.1%
	Property	30.4%	25.0%	34.7%
	Drugs	26.7%	29.5%	24.4%
	Public order	18.1%	17.3%	18.7%

Table 4-2. Bivariate odds ratios for each sentence level for Black offenders

Covariate	Crime	(a) Less than 1 year	(b) 1-1.99 years	(c) 2-4.99 years	(d) 5-9.99 years	(e) 10-24.99 years	(f) More than 25 years	(g) Life or Death sentence
Black	All	1.04	0.87	1	0.97	1.1	1.38	1.57
	Violent	0.79	0.75	0.97	1.04	1.1	1.15	1.24
	Property	1.25	0.94	1.02	0.89	0.78	1.18	2.25
	Drugs	1.13	0.9	1.13	0.9	0.79	1.2	1.52
	Public offense	1.08	1	0.95	0.96	1.02	1.26	1.38

Note: Odds ratios from logistic regressions with sentence level as outcome and race as the only covariate. Each row is a separate model.

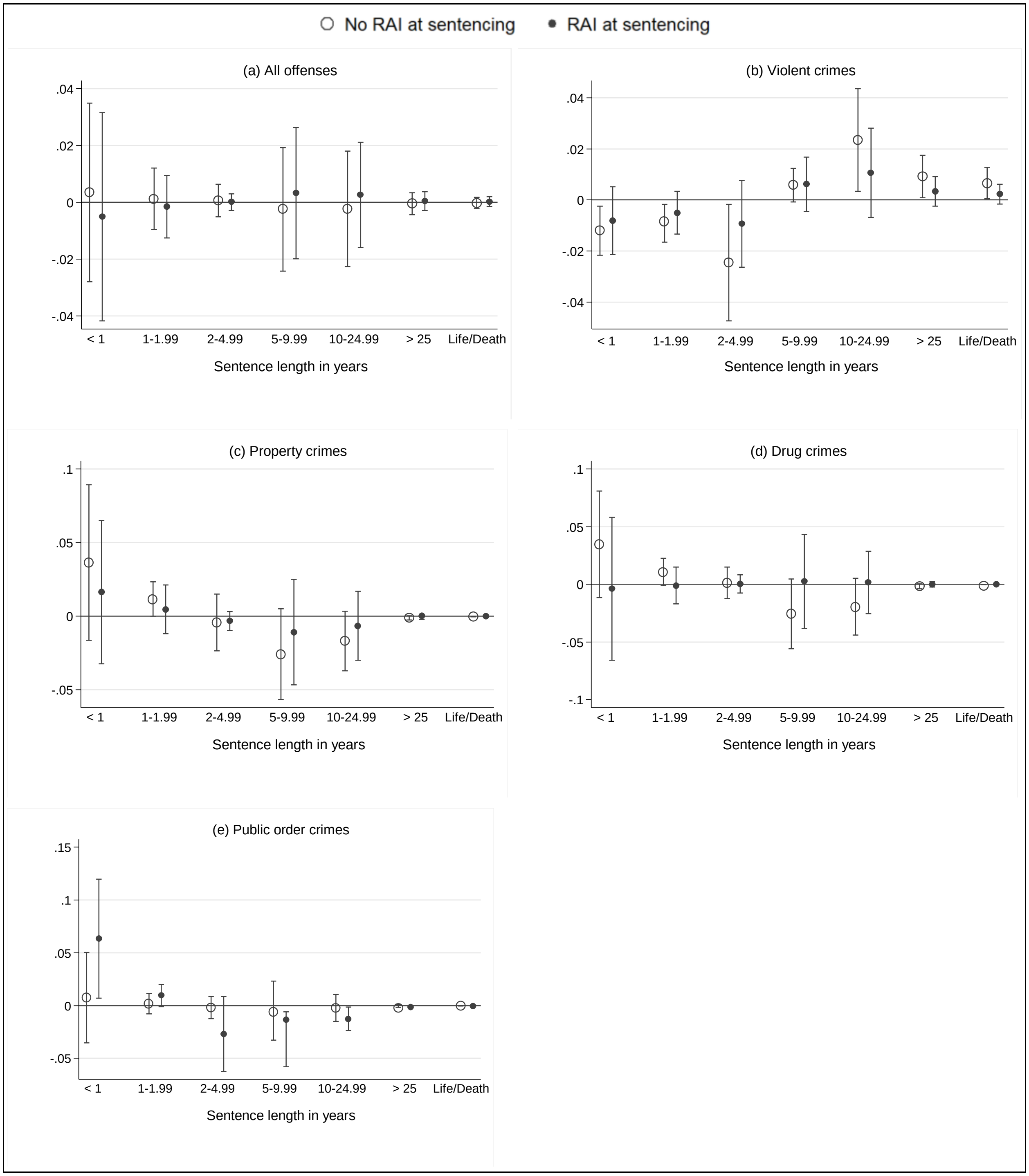
Table 4-3. Conditional odds ratios for independent variables of interest

	(1) All	(2) Violent	(3) Property	(4) Drugs	(5) Public order
RAI policy	0.79	0.71	0.85	0.74	-0.18
	(0.265)	(.204)	(.319)	(.316)	(.331)
Black	0.98	1.17	0.8	0.81	-0.04
	(.105)	(.091)*	(.109)	(.104)	(.119)
RAI*Black	1.06	0.939	1.13	1.26	-0.26
	(.154)	(.103)	(.223)	(.255)	(.173)

Note: Results of ordered logistic regressions with offender-level controls. Robust standard errors in parentheses, clustered by state. *p<.05. Each column is a separate model including only one crime type.

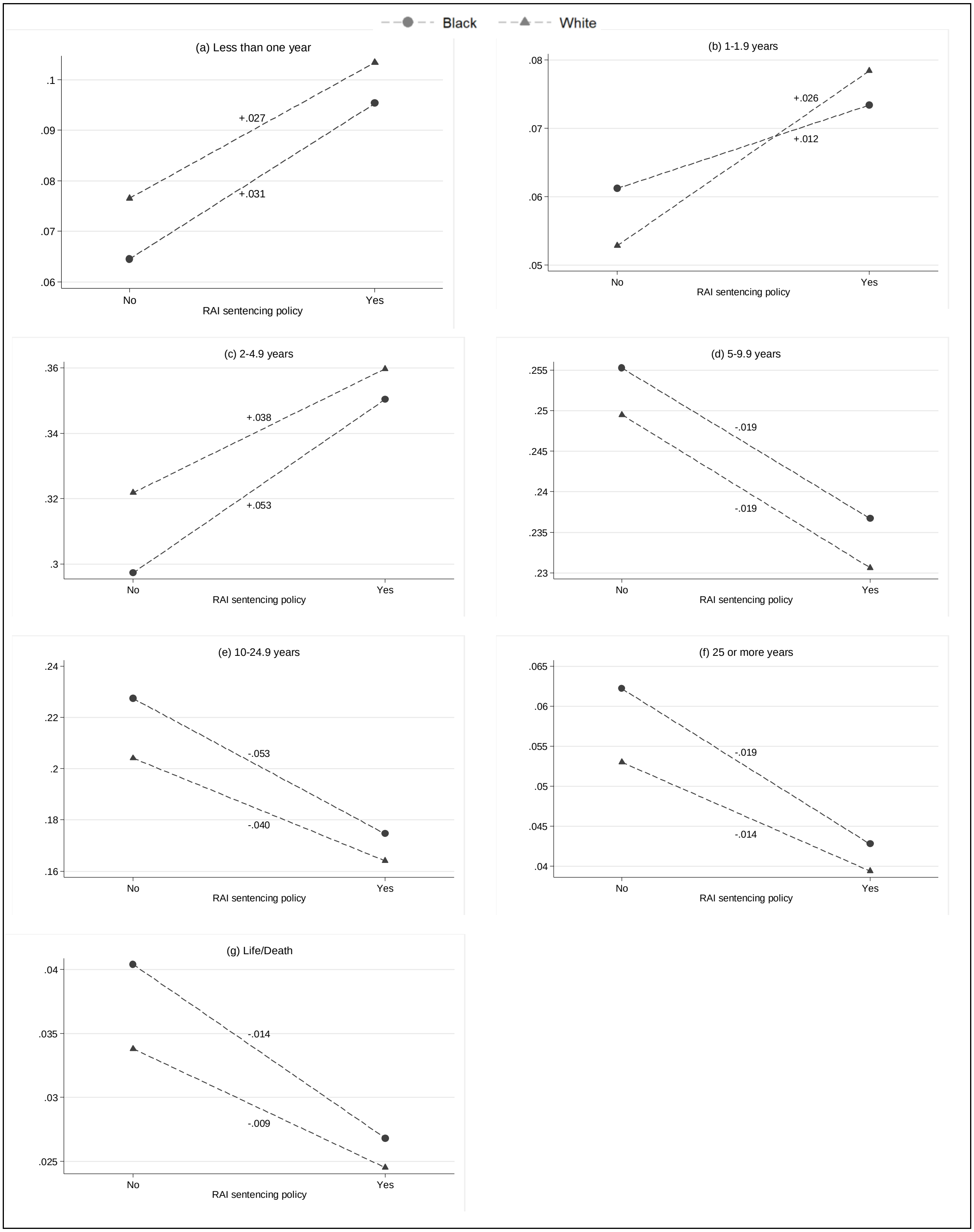
4.6 Figures

Figure 4-1. Differences between races in predicted probabilities at each sentence length by offense



Note: Black/White difference in predicted probability from ordered logistic regression estimates with 95% confidence intervals. Separate models for each offense. Open circles are differences without RAI sentencing policy at the time of admission. Solid circles are differences with RAI sentencing policy at the time of admission.

Figure 4-2. Change in predicted probability of receiving sentence for violent crime for each race with and without RAI policy



Note: Predicted probabilities for violent crimes sentencing for Black offenders (circles) and White offenders (triangles) without policy (left) and with (right) and change along dashed line.

APPENDIX A

Table A-1. State risk assessment policy components

(a) State	(b) Year	(c) Bill	(d) Pretrial	(e) Sentencing	(f) Parole	(g) Probation	(h) Services while incarcerated	(i) Community based, diversion, or reentry
AK	2016	SB 91	X					
AL	2015	SB 67			X	X		X
AR	2011	SB 750 HB1663			X	X		
CA	2009 2018	SB 18 SB 678 SB 10	X		X	X		
CO	2010 2013	HB 1374 SB 250			X	X		
CT	2015	SB 796			X			
DE	2011	SB 226	X					
GA	2012 2015	HB 1176 HB 310			X	X	X	X
HI	2012	SB 2866	X		X			X
IA	2016	HB 2064			X			
ID	2014	SB 1357			X	X		
IN	2017	HB 1137	X					
IL	2009 2017	SB 1289 SB 1607			X		X	
KS	2000	SB 323		X				
KY	2011	HB 463	X	X	X	X		
LA	2011 2013	SB 202 SB 94			X			
MD	2016 2018	SB 1005 SB 812	X		X			
MI	2014 2017	HB 5929 SB 16			X			
MO	2018	HB 1355			X	X	X	
MS	2014	HB 585			X	X		X
MT	2017	SB 60	X	X	X	X		
NC	2011	HB 642				X		
NE	2006 2015	LB 1199 LB 605			X	X		
NH	2010 2016	SB 500 SB 464	X		X	X		
NJ	2014	SB 946	X					
NV	2015	AB 225						X

Table A-1 continued. State RAI policy components

(a) State	(b) Year	(c) Bill	(d) Pretrial	(e) Sentencing	(f) Parole	(g) Probation	(h) Services while incarcerated	(i) Community based, diversion, or reentry
OH	2011	HB 86 SB 603		X	X	X	X	X
OK	2017 2018	SB603			X		X	
PA	2008	HB 4			X			
RI	2017	HB 5064 HB 5056	X		X	X	X	
SC	2009	SB 1154			X	X		
SD	2013 2017	SB 69 SB 70 HB1183				X		
TN	2009 2012 2016 2018	SB 104 HB 2248 HB2576 HB 2181		X	X	X		X
TX	2007 2013 2017	SB 166 SB 213 SB 1584			X	X		X
UT	2015 2016	HB 348 HB 3004		X		X	X	
VA	1994	HB 5001		X				
VT	2014	SB 295	X					
WA	1999	SB 5421						
WI	2011	SB 104						
WV	2013	SB 371	X					
VA	1994	HB500		X				

Note: Column (b) is the year in which the policy was adopted, and column (c) is the bill number. If multiple bills were passed, all years and bill numbers are included. Columns (e) – (i) display an "X" when the law includes the policy component in the heading[17]. For example, Alaska senate bill 91 passed in 2016 and legislated the use of pretrial risk assessments

[17] The table does not distinguish between components included in separate bills.

APPENDIX B

Table B-1. Predicted probabilities of receiving sentence in the absence of RAI sentencing policy

Crime type	(1) Less than 1 year	(2) 1-1.9 years	(3) 2-4.9 years	(4) 5-9.9 years	(5) 10-24.9 years	(6) More than 25 years	(7) Life or Death sentence
All							
Black	18.3%	10.0%	36.1%	21.0%	11.7%	1.9%	1.0%
	(.043)	(.018)	(.027)	(.022)	(.022)	(.003)	(.002)
White	18.0%	9.8%	36.1%	21.3%	12.0%	1.9%	1.0%
	(.035)	(.019)	(.027)	(.036)	(.021)	(.002)	(.002)
Violent							
Black	6.5%	6.1%	29.7%	25.5%	22.8%	6.2%	4.0%
	(.018)	(.013)	(.027)	(.013)	(.027)	(.008)	(.006)
White	7.7%	5.3%	32.2%	25.0%	20.4%	5.3%	3.4%
	(.019)	(.015)	(.028)	(.015)	(.024)	(.006)	(.004)
Property							
Black	23.4%	12.7%	37.9%	17.9%	7.7%	0.5%	0.1%
	(.065)	(.023)	(.041)	(.031)	(.022)	(.001)	(.0002)
White	19.7%	11.5%	38.3%	20.4%	9.4%	0.6%	0.1%
	(.045)	(.023)	(.036)	(.028)	(.024)	(.001)	(.0002)
Drug							
Black	22.5%	11.2%	35.6%	20.5%	9.5%	0.7%	0.1%
	(055)	(.021)	(.031)	(.027)	(.019)	(.002)	(.0002)
White	19.0%	10.1%	35.5%	23.1%	11.4%	0.9%	0.1%
	(.043)	(.020)	(.029)	(.028)	(.024)	(.002)	(.0003)
Public Order							
Black	25.0%	11.2%	40.1%	17.2%	5.8%	0.6%	0.1%
	(.048)	(.021)	(.033)	(.023)	(.013)	(.002)	(.0003)
White	24.2%	11.0%	40.3%	17.7%	6.0%	0.6%	0.1%
	(.046)	(.023)	(.033)	(.025)	(.013)	(.002)	(.0004)

Note: Predicted probabilities from ordered logistic regressions when RAI sentencing policy = 0. Robust standard errors in parentheses. Rows in crime type “All” include all observations, followed by separate models for each crime type.

Table B-2. Predicted probabilities of receiving sentence with RAI sentencing policy

Crime type	(1) Less than 1 year	(2) 1-1.9 years	(3) 2-4.9 years	(4) 5-9.9 years	(5) 10-24.9 years	(6) More than 25 years	(7) Life or Death sentence
All							
Black	21.0%	10.8%	36.4%	19.3%	10.2%	1.6%	0.8%
	(.056)	(.024)	(.028)	(.033)	(.027)	(.005)	(.002)
White	21.5%	11.0%	36.4%	18.9%	9.9%	1.6%	0.8%
	(.215)	(.025)	(.029)	(.036)	(.028)	(.005)	(.002)
Violent							
Black	9.5%	7.3%	35.0%	23.7%	17.5%	4.3%	2.7%
	(.029)	(.019)	(.037)	(.022)	(.031)	(.010)	(.006)
White	10.3%	7.8%	36.0%	23.1%	16.4%	3.9%	2.5%
	(.031)	(.020)	(.038)	(.023)	(.030)	(.009)	(.006)
Property							
Black	24.0%	12.8%	37.7%	17.4%	7.5%	0.5%	0.7%
	(.054)	(.025)	(.039)	(.028)	(0.19)	(.001)	(.0002)
White	22.4%	12.4%	38.0%	18.5%	8.1%	0.5%	0.7%
	(.069)	(.029)	(.039)	(.043)	(.028)	(.002)	(.0003)
Drug							
Black	23.6%	11.5%	35.5%	19.7%	9.0%	0.7%	0.1%
	(.081)	(.028)	(.032)	(.049)	(.033)	(.003)	(.0003)
White	24.0%	11.6%	35.5%	19.5%	8.8%	0.7%	0.1%
	(.083)	(.028)	(.032)	(.051)	(.034)	(.003)	(.0003)
Public Order							
Black	34.0%	12.7%	36.6%	12.4%	3.8%	0.4%	0.1%
	(.068)	(.027)	(.041)	(.033)	(.014)	(.001)	(.0003)
White	27.7%	11.8%	39.3%	15.6%	5.1%	0.5%	0.1%
	(.056)	(.026)	(.033)	(.034)	(.016)	(.002)	(.0004)

Note: Predicted probabilities from ordered logistic regressions when RAI sentencing policy = 1. Standard errors in parentheses. Rows in crime type "All" include all observations, followed by separate models for each crime type.

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